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15

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May 1864
R.H.

Dolland

Engraved by J. Posselwhite

DOLLOND

From an original Picture in the Royal Observatory, Greenwich.

Under the Superintendance of the Society for the Diffusion of Useful Knowledge

London, Published by Chas. Knight Ludgate Street & Pall Mall East

THE LIFE OF JOHN DOLLOND, F.R.S.

INVENTOR OF THE Achromatic Telescope.

With a copious Appendix of all the Papers referred to.

BY

JOHN KELLY, LL.D.

RECTOR OF COPFORD, IN THE COUNTY OF ESSEX;

Author of the Triglott Celtic Dictionary, and a Translator of the Bible into the Manks Gaelic.

THIRD EDITION WITH ADDITIONS.

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CONTENTS:

Page

The Life of John Dollond, F.R.S. 5

A Letter from Mr. John Dollond *to Mr.* James Short, F.R.S. *concerning an Improvement of* Refracting Telescopes 17

Letters relating to a Theorem of M. Euler, *of the Royal Academy of Sciences at Berlin, and* F.R.S. *for correcting the Aberrations in the* Object-Glasses *of* Refracting Telescopes 21

A Description of a Contrivance for Measuring Small Angles, *by Mr.* John Dollond; *communicated by Mr.* J. Short, F.R.S. . 33

An Explanation of an Instrument for Measuring Small Angles, *the first Account of which was read before the Royal Society, May* 10, 1753. *By Mr.* John Dollond. *In a Letter to* James Short, M.A. *and* F.R.S. 36

A

Page

An Account of some Experiments concerning the different Refrangibility of Light. *By Mr.* John Dollond. *With a Letter from* James Short, M.A. F.R.S. *Acad. Reg. Suec. Soc.* 50

Some Account of the Discovery, made by the late Mr. John Dollond, F.R.S. *which led to the grand Improvement of* Refracting Telescopes, *in order to correct some Misrepresentations, in Foreign Publications, of that Discovery: with an Attempt to account for the* Mistake *in an Experiment made by Sir* Isaac Newton; *on which Experiment, the Improvement of the* Refracting Telescope *intirely depended, By* Peter Dollond, *Member of the American Philosophical Society at Philadelphia* 61

An Attempt to explain a Difficulty in the Theory of Vision, *depending on the different* Refrangibility of Light. *By the Rev.* Nevil Maskelyne, D.D. F.R.S. *and Astronomer Royal* 78

An Account of an Improvement made by Mr. Peter Dollond *in his* New Telescopes. *In a Letter to* James Short, M.A. F.R.S. *with a Letter of Mr.* Short *to the Rev.* Thomas Birch, D.D. *Sec.* R.S. 88

A Letter from Mr. Peter Dollond, *to* Nevil Maskelyne, F.R.S. *and Astronomer Royal; describing some Additions and Alterations made to* Hadley's Quadrant, *to render it more serviceable at Sea* . . 92

Remarks on the Hadley's Quadrant, *tending principally to remove the Difficulties which have hitherto attended the Use of the Back-observa-*

Page

tion, and to obviate the Errors that might arise from a Want of Parallelism in the two Surfaces of the Index-Glass. By Nevil Maskelyne, F.R.S. *Astronomer Royal* 96

An Account of an Apparatus applied to the Equatorial Instrument for correcting the Errors arising from the Refraction in Altitude. *By Mr.* Peter Dollond, *Optician; communicated to the Royal Society by the Astronomer Royal* 119

THE

LIFE

OF

JOHN DOLLOND, F.R.S.

&c.

IN modern times the attention of men has been employed rather in improving what they know than in attempting to make new discoveries. When a man, therefore, has been fortunate enough, by extraordinary research, or by a strong effort of genius, to surprise the world with a new invention, a lively interest is immediately excited in every mind to trace the steps, investigate the means, and collect

every incident which led to the result:—and to the honour of human nature be it said, while curiosity exerts itself in this manner on the invention, the inventor is not less the object of regard and consideration; we wish to learn the history, the life, the character of the man, and, as far as it is possible, to be acquainted with him. The subject of the following memoir is entitled to this introduction, and the public will receive with satisfaction the following account of the inventor of the achromatic telescope.

John Dollond, fellow of the Royal Society, was born in Spitalfields,
1706 on the tenth day of June in the year 1706: his parents were French protestants, and at the time of the revocation of the edict of Nantz, which happened in the year 1685, resided in Normandy; but in what particular part of it is not at present precisely known: M. de Lalande does not believe the name to be of French origin: but however this may be, the family were compelled soon after this period to seek refuge in England, in order to avoid persecution and to preserve their religion.

The fate of this family was not a solitary case; fifty thousand persons pursued the same measures, and we may date from this period the rise of several arts and manufactures, which have become highly beneficial to this country. An establishment was given to these refugees, by the wise policy of our government, in Spitalfields, and particular encouragement granted to the silk manufactory.

The first years of Mr. Dollond's life were employed at the loom; but, being of a very studious and philosophic turn of mind, his leisure hours were engaged in mathematical pursuits; and though by the death of his father, which happened in his infancy, his education gave way to the necessities of his family, yet at the age of fifteen,

before he had an opportunity of seeing works of science or elementary treatises, he amused himself by constructing sun-dials, drawing geometrical schemes, and solving problems.

An early marriage and an increasing family afforded him little opportunity of pursuing his favourite studies: but such are the powers of the human mind when called into action, that difficulties, which appear to the casual observer insurmountable, yield and retire before perseverance and genius: even under the pressure of a close application to business for the support of his family, he found time, by abridging the hours of his rest, to extend his mathematical knowledge, and made a considerable proficiency in optics and astronomy, to which he now principally devoted his attention, having in the earlier stages of his life prepared himself for the higher parts of those subjects by a perfect knowledge of algebra and geometry.

Soon after this, without abating from the ardour of his other literary pursuits, or relaxing from the labours of his profession, he began to study anatomy, and likewise to read divinity; and finding the knowledge of Latin and Greek indispensably necessary towards attaining those ends, he applied himself diligently, and was soon able to translate the Greek Testament into Latin; and as he admired the power and the wisdom of the Creator in the mechanism of the human frame, so he adored his goodness displayed in his revealed word.

It might from hence be concluded that his sabbath was devoted to retired reading and philosophical objects; but he was not content with private devotion, as he was always an advocate for social worship, and with his family regularly attended the public service of the French

protestant church, and occasionally heard Benson and Lardner, whom he respected as men and admired as preachers. In his appearance he was grave, and the strong lines of his face were marked with deep thought and reflection; but in his intercourse with his family and friends, he was cheerful and affectionate; and his language and sentiments are distinctly recollected as always making a strong impression on the minds of those with whom he conversed. His memory was extraordinarily retentive, and, amidst the variety of his reading, he could recollect and quote the most important passages of every book which he had at any time perused.

He designed his eldest son, Peter Dollond, for the same business with himself; and for several years they carried on their manufactures together in Spitalfields; but the employment neither suited the expectations nor disposition of the son, who, having received much information upon mathematical and philosophical subjects from the instruction of his father, and observing the great value which was set upon his father's knowledge in the theory of optics by professional men, determined to apply that knowledge to the benefit of himself and his family; and accordingly, under the directions of his father, commenced optician. Success, though under the most unfavourable circumstances, attended every effort; and in the year 1752 John Dollond, embracing the opportunity of pursuing a profession congenial with his mind, and without neglecting the rules of prudence towards his family, joined his son, and in consequence of his theoretical knowledge, soon became a proficient in the practical parts of optics.

His first attention was directed to improve the combination of the eye-glasses of refracting telescopes; and having succeeded in his sys-

tem of four eye-glasses, he proceeded one step further, and produced telescopes furnished with five eye-glasses, which considerably surpassed the former; and of which he gave a particular account in a paper presented to the Royal Society, and which was read on the 1st of March 1753, and printed in the Philosophical Transactions, vol. xlviii. page 108.—*See Appendix, pp.* 17—20.

Soon after this he made a very useful improvement in Mr. Savery's micrometer: for instead of employing two entire object-glasses, as Mr. Savery and M. Bouguer had done, he used only one glass cut into two equal parts, one of them sliding or moving laterally by the other. This was considered to be a great improvement, as the micrometer could now be applied to the reflecting telescope with much advantage, and which Mr. James Short immediately did. An account of the same was given to the Royal Society, in a paper which was afterwards printed in the Philosophical Transactions, vol. xlviii. page 178; and in another paper, part ii. page 551.—*See Appendix, pp.* 33—49.*

Mr. Dollond's celebrity in optics became now universal; and the friendship and protection of the most eminent men of science flattered and encouraged his pursuits. To enumerate the persons, both at home and abroad, who distinguished him by their correspondence or cultivated his acquaintance, however honourable to his memory, would only be an empty praise. We cannot, however, forbear men-

* This kind of micrometer was afterwards applied by Mr. P. Dollond to the achromatic telescope.—*See Appendix, pp.* 88—91.

tioning the names of a few persons, who held the highest place in his esteem as men of worth and learning:—Mr. Thomas Simpson, master of the Royal Academy at Woolwich; Mr. Harris, assay-master at the Tower, who was at that time engaged in writing and publishing his Treatise on Optics; the Rev. Dr. Bradley, then astronomer royal; the Rev. William Ludlam, of St. John's college, Cambridge; Mr. John Canton, a most ingenious man, and celebrated not less for his knowledge in natural philosophy, than for his neat and accurate manner of making philosophical experiments. To this catalogue of the philosophical names of those days, we must add that of the present astronomer royal, the Rev. Dr. Maskelyne, whose labours have so eminently benefited the science of astronomy.

Surrounded by these enlightened men, in a state of mind prepared for the severest investigation of philosophic truths, and in circumstances favourable to liberal inquiry, Mr. Dollond engaged in the discussion of a subject, which at that time not only interested this country, but all Europe. Sir Isaac Newton had declared, in his Treatise on Optics, page 112, " That all refracting substances diverged the prismatic colours in a constant proportion to their mean refraction;" and drew this conclusion, " that refraction could not be produced without colour;" and consequently, " *that no improvement could be expected in the refracting telescope*." No one doubted the accuracy with which Sir Isaac Newton had made the experiment; yet some men, particularly M. Euler and others, were of opinion that the conclusion which Newton had drawn from it went too far, and maintained that in very small angles refraction might be obtained without colour. Mr. Dollond was not of that opinion, but defended

Newton's doctrine with much learning and ingenuity, as may be seen by a reference to the letters which passed between Euler and Dollond upon that occasion, and which were published in the Philosophical Transactions, vol. xlviii. page 287—*see Appendix, pp.* 21—32; and contended that, "If the result of the experiment had been as described by Sir Isaac Newton, there could not be refraction without colour."

A mind constituted like Mr. Dollond's could not remain satisfied with arguing in this manner from an experiment made by another, but determined to try it himself, and accordingly, in the year 1757, began the examination; and, to use his own words, with, "a resolute perseverance," continued during that year, and a great part of the next, to bestow his whole mind on the subject, until in the month of June 1758 he found, after a complete course of experiment, the result to be very different from that which he expected, and from that which Sir Isaac Newton had related. He discovered "*the difference in the dispersion of the colours of light, when the mean rays are equally refracted by different mediums.*" The discovery was complete, and he immediately drew from it this practical conclusion, "That the object-glasses of refracting telescopes were capable of being made without the images formed by them being affected by the different refrangibility of the rays of light." His account of this experiment, and of others connected with it, was given to the Royal Society, and printed in their Transactions, vol. l. page 743—*see Appendix, pp.* 50—60; and he was presented in the same year, by that learned body, with Sir Godfrey Copley's medal, as a reward of his merit, and a memorial of the discovery, though not at that time a member of the society.

This discovery no way affected the points in dispute between Euler and Dollond, respecting the doctrine advanced by Sir Isaac Newton.* A new principle was in a manner found out, which had no part in their former reasonings, and it was reserved for the accuracy of Dollond to have the honour of making a discovery which had eluded the observation of the immortal Newton.†

This new principle being now established, he was soon able to construct object-glasses, in which the different refrangibility of the rays of light was corrected, and the name of achromatic given to them by the late Dr. Bevis, on account of their being free from the prismatic colours. Dr. Hutton, in his Mathematical Dictionary, has said that this name was given to them by M. de Lalande; but that is a mistake.

As usually happens on such occasions, no sooner was the achromatic telescope made public, than the rivalship of foreigners, and the jealousy of philosophers at home, led them to doubt of its reality; and Euler himself, in his paper read before the Academy of Sciences at Berlin, in the year 1764, says, "I am not ashamed frankly to avow, that the first accounts, which were published of it, appeared so suspicious, and even so contrary to the best established principles, that I could not prevail upon myself to give credit to them;"

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* See note at bottom of pages 79—80, for Priestley's remarks, &c.

† The cause of this difference of the results of the 8th experiment of the 2nd part of the first book of Newton's Optics, as related by himself, and as it was found when tried by Dollond in the years 1757 and 1758, is fully and ingeniously accounted for by Mr. Peter Dollond in a paper read at the Royal Society on the 21st of May 1789, and afterwards published for J. Johnson in St. Paul's Church Yard—*see Appendix, pp.* 61—77; also in Hutton's Dictionary—Article, Chromatic.

and he adds, " I should never have submitted to the proofs which Mr. Dollond produced to support this strange phænomenon, if M. Clairaut, who must at first have been equally surprized at it, had not most positively assured me that Dollond's experiments were but too well founded." And when the fact could no longer be disputed, they endeavoured to find a prior inventor, to whom it might be ascribed, and several conjecturers were honoured with the title of discoverers.

Mr. Dollond's improvement in refracting telescopes was of the greatest advantage in astronomy, as they have been applied to fixed instruments; by which the motions of the heavenly bodies are determined to a much greater exactness than by the means of the old telescope. Navigation has also been much benefited by applying achromatic telescopes to the " Hadley's sextant:" and from the improved state of the lunar tables, and of that instrument, the longitude at sea may now be determined by good observers to a great degree of accuracy; and their universal adoption by the navy and army, as well as by the public in general, is the best proof of the great utility of the discovery.

In the beginning of the year 1761, Mr. Dollond was elected fellow of the Royal Society, and appointed optician to his majesty, but did not live to enjoy those honours long; for on the 30th of November, in the same year, as he was reading a new publication of M. Clairaut, on the theory of the moon, and on which he had been intently engaged for several hours, he was seized with apoplexy, which rendered him immediately speechless, and occasioned his death in a few hours afterwards. Besides Mr. Peter Dollond, whom we had occasion to mention in this memoir, his family, at his death, consisted of three

daughters and a son, who, possessing the name of his father, and we may add, a portion of the family abilities, carried on the optical business in partnership with his elder brother.

Since the last edition of this Life, we have to mention the death of Mr. John Dollond, the partner of his elder brother Mr. Peter Dollond, which has occasioned the latter to take into partnership his nephew, the son of his eldest sister, Mr. George Huggins, who has, by the King's permission, taken the name of DOLLOND.

THE END OF THE LIFE.

THE APPENDIX.

APPENDIX.

A Letter from Mr. John Dollond *to Mr.* James Short, F.R S. *concerning an Improvement of* Refracting Telescopes.

Read March 1, 1753.

SIR,

IT is well known, that the perfection of refracting telescopes is very much limited by the aberration of the rays of light from the geometrical focus; which arises from two very different causes; that is, from different degrees of refrangibility of light, and from the figure of the sphere, which is not of a proper curvature for collecting the rays in a single point. The object-glass is chiefly affected by the first of these; nor has there been yet any method discovered for rectifying that aberration so, as in the least to remove the indistinctness of the image arising from it. We are therefore reduced to the necessity of contracting their apertures, which renders it impossible to magnify much without very long glasses.

But the case is widely different with regard to the eye-glasses; for, though they are very much affected by both the aberrations before-mentioned, yet, by a proper combination of several together, their errors may be in a great measure corrected. If any one, for instance, would have the visual angle of a telescope to contain 20 degrees, the extreme pencils of the field must be bent or refracted in an angle of 10 degrees; which, if it be performed by one eye-glass, will cause an aberration from the figure, in proportion to the cube of that angle: but if two glasses are so proportioned and situated, as that the refraction may be equally divided between them, they will each of them produce a refraction equal to half the required angle: and therefore the aberration being in proportion to the cube of half the angle taken twice over, will be but a fourth part of that, which is in proportion to the cube of the whole angle; because twice the cube of one is but $\frac{1}{4}$ of the cube of two; so the aberration from the figure, where two eye-glasses are rightly proportioned, is but a fourth of what must unavoidably be, where the whole is performed by a single eye-glass. By the same way of reasoning, when the refraction is divided between three glasses, the aberration will be found to be but the ninth part of what would be produced from a single glass; because three times the cube of one is but one ninth of the cube of 3. Whence it appears, that, by increasing the number of eye-glasses, the indistinctness, which is observed near the borders of the field of a telescope, may be very much diminished, though not intirely taken away.

The method of correcting the errors arising from the different refrangibility of light is of a different consideration from the former; for, whereas the errors from the figure can only be diminished in a

certain proportion to the number of glasses, in this they may be intirely corrected, by the addition of only one glass; as we find in the astronomical telescope, that two eye-glasses, rightly proportioned, will cause the edges of objects to appear free from colours quite to the borders of the field. Also in the day telescope, where no more than two eye-glasses are absolutely necessary for erecting the object, we find, by the addition of a third rightly situated, that the colours, which would otherwise confuse the image, are intirely removed:—I say intirely removed; but this is to be understood with some limitation; for though the different colours, which the extreme pencils must necessarily be divided into by the edges of the eye-glasses, may in this manner be brought to the eye in a direction parallel to each other, so as, by the humours thereof, to be converged to a point in the retina; yet, if the glasses exceed a certain length, the colours may be spread too wide to be capable of being admitted through the pupil or aperture of the eye; which is the reason, that, in long telescopes, constructed in the common manner, with three eye-glasses, the field is always very much contracted.

These considerations, Sir, first set me on contriving, how to enlarge the field by increasing the number of eye-glasses, without any hinderance to the distinctness or brightness of the image: and though others had been about the same work before, yet observing, that the five-glass telescopes, sold in the shops, would admit of farther improvement, I endeavoured to construct one with the same number of glasses in a better manner; which so far answered my expectations, as to be allowed by such persons, as are the best judges, to be a considerable improvement on the former.

Encouraged by this success, I resolved to try, if possibly I might gain some farther enlargement of the field by the addition of another glass: and by placing and proportioning the glasses in such a manner, as to correct the aberrations as much as possible, without any detriment to the distinctness, I have obtained as large a field, as is convenient or necessary, and that even in the longest telescopes, which can be made.

These telescopes with six glasses having been well received, and some of them being gone to foreign parts, it seems a proper time to settle the account of its origin; which is one of the motives, that has induced me to trouble you with this short sketch of the considerations, that gradually led me to its construction; and I am emboldened, Sir, to write thus much, from the many favours I have already received at your hands, as well as from a sense of your being a proper person to judge in such cases. And though I am sensible, that you are not unacquainted with the theory contained in this letter, yet forasmuch as the subject has never been fully treated by any author, I shall endeavour, as soon as may be, to draw up a more particular explanation of the aberrations of light by refraction; but shall add no more at present, only beg leave to take this opportunity of subscribing myself

Your much obliged

and most humble servant,

John Dollond.

Vine Court,
February 21, 1753.

Letters relating to a Theorem of Mr. Euler, *of the* Royal Academy *of* Sciences *at* Berlin, *and* F.R.S. *for correcting the Aberrations in the* Object-Glasses *of* Refracting Telescopes.

No. 1.

A Letter from Mr. James Short, F.R.S. *to* Peter Duval, *Esq.* F.R.S.

Read April 9, 1752.

DEAR SIR,

THERE is published, in the Memoirs of the Royal Academy at Berlin, for the year 1747, a theorem by Mr. Euler, in which he shews a method of making object-glasses of telescopes, in such a manner, as not to be affected by the aberrations arising from the different refrangibility of the rays of light: these object-glasses consisting of two *meniscus* lenses, with water between them.

Mr. John Dollond, who is an excellent analyst and optician, has examined the said theorem, and has discovered a mistake in it, which arises by assuming an hypothesis contrary to the established principles of optics; and, in consequence of this, Mr. Dollond has sent me

the inclosed letter, which contains the discovery of the said mistake, and a demonstration of it.

In order to act in the most candid manner with Mr. Euler, I have proposed to Mr. Dollond to write to him, shewing him the mistake, and desiring to know his reasons for that hypothesis; and therefore I desire, that this letter of Mr. Dollond's to me may be kept amongst the Society's papers, till Mr. Euler has had a sufficient time to answer Mr. Dollond's letter to him.

I am, SIR,

Your most humble servant,

James Short.

Surrey Street,
April 9, 1752.

No. 2.

A Letter from Mr. John Dollond *to* James Short, A.M. F.R.S. *concerning a Mistake in M.* Euler's *Theorem for correcting the Aberrations in the Object-Glasses of* Refracting Telescopes.

Read November 23, 1752.

SIR,

THE famous experiments of the prism, first tried by Sir Isaac Newton, sufficiently convinced that great man, that the perfection of telescopes was impeded by the different refrangibility of the rays of light, and not by the spherical figure of the glasses, as the common notion had been till that time; which put the philosopher upon grinding concave metals, in order to come at that by reflection, which he despaired of obtaining by refraction. For, that he was satisfied of the impossibility of correcting that aberration by a multiplicity of refractions, appears by his own words, in his Treatise of Light and Colours, *Book* I. *Part* 2. *Prop.* 3. "I found more-
" over, that when light goes out of air through several contiguous
" mediums, as through water and glass, as often as by contrary

"refractions it is so corrected, that it emergeth in lines parallel to "those in which it was incident, continues ever after to be white. "But if the emergent rays be inclined to the incident, the whiteness "of the emerging light will by degrees, in passing on from the place "of emergence, become tinged in its edges with colours."

It is therefore, Sir, somewhat strange, that any body now-a-days should attempt to do that, which so long ago has been demonstrated impossible. But, as so great a mathematician as Mr. Euler has lately published a theorem * for making object-glasses, that should be free from the aberration arising from the different refrangibility of light, the subject deserves a particular consideration. I have therefore carefully examined every step of his algebraic reasoning, which I have found strictly true in every part. But a certain hypothesis in page 285, appears to be destitute of support either from reason or experiment, though it be there laid down as the foundation of the whole fabrick. This gentleman puts $m:1$ for the ratio of refraction out of air into glass of the mean refrangible rays, and $M:1$ for that of the least refrangible. Also for the ratio of refraction out of air into water of the mean refrangible rays he puts $n:1$, and for the least refrangible $N:1$. As to the numbers, he makes $m=\frac{31}{20}$, $M=\frac{77}{50}$, and $n=\frac{4}{3}$; which so far answer well enough to experiments. But the difficulty consists in finding the value of N in a true proportion to the rest.

Here the author introduces the supposition above-mentioned;

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* Vide Memoirs of the Royal Academy of Berlin for the Year 1747.

which is, that m is the same power of M, as n is of N; and therefore puts $n=m^a$, and $N=M^a$. Whereas, by all the experiments that have hitherto been made, the proportion will come out thus, $m-1 : n-1 :: m-M : n-N$.

The letters fixed upon by Mr. Euler to represent the radii of the four refracting surfaces of his compound object-glass, are $f\ g\ h$ and k, and the distance of the object he expresses by a; then will the focal distance be $=\frac{1}{n(\frac{1}{g}-\frac{1}{h})+m(\frac{1}{f}-\frac{1}{g}+\frac{1}{h}-\frac{1}{k})-\frac{1}{a}-\frac{1}{f}+\frac{1}{k}}$. Now, says he, it is evident, that the different refrangibility of the rays would make no alteration, either in the place of the image, or in its magnitude, if it were possible to determine the radii of the four surfaces, so as to have $n(\frac{1}{g}-\frac{1}{h})+m(\frac{1}{f}-\frac{1}{g}+\frac{1}{k}-\frac{1}{h})=N(\frac{1}{g}-\frac{1}{h})+M(\frac{1}{f}-\frac{1}{g}+\frac{1}{n}-\frac{1}{h})$. And this, Sir, I shall readily grant. But when the surfaces are thus proportioned, the sum of the refractions will be $=0$; that is to say, the emergent rays will be parallel to the incident. For, if $n(\frac{1}{g}-\frac{1}{h})+m(\frac{1}{f}-\frac{1}{g}+\frac{1}{h}-\frac{1}{k})=N(\frac{1}{g}-\frac{1}{h})+M(\frac{1}{f}-\frac{1}{g}+\frac{1}{h}-\frac{1}{k})$, then $n-N(\frac{1}{g}-\frac{1}{h})+m-M(\frac{1}{f}-\frac{1}{g}+\frac{1}{h}-\frac{1}{k})=0$. Also if $n-N : m-M :: n-1 : m-1$, then $n-1(\frac{1}{g}-\frac{1}{h})+m-1(\frac{1}{f}-\frac{1}{g}+\frac{1}{h}-\frac{1}{k})=0$; or otherwise $n(\frac{1}{g}-\frac{1}{h})+m(\frac{1}{f}-\frac{1}{g}+\frac{1}{h}-\frac{1}{k})-\frac{1}{f}+\frac{1}{k}=0$; which reduces the denominator of the fraction expressing the focal distance to $\frac{1}{a}$. Whence the focal distance will be $=a$; or, in other words, the image will be the object itself. And as, in this case, there will be no refraction, it will be easy to conceive how there should be no aberration.

And now, Sir, I think I have demonstrated, that Mr. Euler's theorem is intirely founded upon a new law of refraction of his own; but that, according to the laws discovered by experiment, the

aberration arising from the different refrangibility of light at the object-glass cannot be corrected by any number of refractions whatsoever.

I am, SIR,

Your most obedient humble servant,

John Dollond.

London,
March 11, 1752.

No. 3.

Mr. Euler's *Letter to Mr.* James Short, F.R.S.

Read July 8, 1753.

MONSIEUR,

VOUS m'avez fait un tres sensible plaisir, en ayant disposé M. Dollond de remettre la proposition de ses objections contre mes verres objectifs, jusqu' à ce que j'y aurois repondu, et je vous en suis infiniment obligé. Je prend donc la liberté de vous addresser ma reponse à lui, en vous priant, après l'avoir daignée de votre examen, de la vouloir bien lui remettre: et en cas que vous jugiez cette matiere digne de l'attention de la Societé Royale, je vous prierois de lui communiquer les preuves detaillées de ma theorie, que j'ai exposée dans cette lettre. Cependant j'espere, que M. Dollond en sera satisfait, puisque je tombe d'accord avec lui du peu de succes, qu'on sauroit se promettre de mes objectifs, en les travaillant selon la maniere ordinaire.

J'ai l'honneur d'etre, avec la plus parfaite consideration,

MONSIEUR,

Votre tres humble, et

tres obéissant serviteur,

L. Euler.

Berlin,
Juin 19, 1752.

No. 4.

A Monsieur Monsieur Dollond.

Read July 8, 1753.

MONSIEUR,

ETANT tres sensible à l'honneur que vous me faites, au sujet des verres objectifs, que j'avois proposé, j'ai celui de vous marquer d'abord ingenument, que j'ai rencontré aussi ici le plus grands obstacles dans l'execution de ce dessein, vu qu'il s'agit de quatre faces, qui doivent etre travaillée exactement selon les proportions que j'avois trouvées: cependant ayant fait les experiences sur quelquesuns, qui parurent le mieux reussi, nous avons trouvé, que l'intervalle entre les deux foyers des rayons rouges et violets etoit beaucoup plus petit, qu'il ne seroit d'un verre simple de la meme distance focale. Neantmoins je dois avoüer, qu'un tel verre, quand même il bien seroit parfaitement executé sur mes principes, auroit d'autres defauts, qui le mettroient au dessous même des verres ordinaires: c'est qu'un tel verre n'admet qu'un tres petite ouverture en consequence des grandes courbures, qu'on doit donner aux faces interieures: desorte que lorsqu'on donne une ouverture ordinaire, l'image devient tres confus.

Ainsi puisque vous vous etes donné la peine, Monsieur, d'executer de tels verres, en en faisant des experiences*, je vous prie de bien distinguer les defauts, qui peuvent naitre de la diverse refrangibilité des rayons, de ceux, qui viennent d'une trop grande ouverture: pour cet effet vous n'aurez qu'à laisser une tres petite ouverture.

Or si ma theorie etoit juste, dont j'aurai bientot l'honneur de parler, il seroit moyen de remedier à ce defaut; il faudroit renoncer à la figure spherique qu'on donne ordinairement aux faces des verres, et tacher de leur donner une autre figure, et j'ai remarqué que la figure d'une parabole leur procureroit l'avantage, qu'ils admettroient une ouverture tres considerable. Notre savant M. Lieberkuhn s'est appliqué à travailler des verres dont la courbure des faces décroit depuis le milieu vers le bords, et il s'en est aperçu de tres grands avantages. Par ces raisons je crois, que ma theorie ne souffre encore rien de ce cotè.

Pour la theorie, je conviens avec vous, monsieur, que posant la raport de refraction d'un milieu dans un autre quelconque pour les rayons moyens comme m à 1, et pour les rayons rouges comme M à 1, la raison de $m-M$ à $m-1$ sera toujours si à peu prés constant, qu'elle satisfera à toutes les experiences, comme la grand Newton a remarqué. Cette raison ne differe non plus de ma theorie que presque imperceptiblement: car puisque je soutiens que $M=m^{a}$, et que m differe ordinairement fort peu de l'unité, soit $m=1+\omega$; et

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* Mr. Dollond, in his letter to Mr. Euler, here referred to, does not say that he had made any trials himself, but only he had understood that such had been made by others, without success.

puisque $M = m^{\alpha} = 1 + a\, l\, m$ à peu pres, et $l\ (1+\omega) = lm = \omega$, aussi fort à peu pres, j'aurai $m - M = 1 + \omega - 1 - a\omega = (1-\alpha)\ \omega$, et $m - 1 = \omega$, donc la raison $\frac{m-M}{m-1}$ sera $= 1 - \alpha$, ou fort à peu pres constante. Delà je conclud, que les experiences d'ou le grand Newton a tiré son raport, ne sauroient etre contraires à ma theorie.

En second lieu, je conviens aussi que si la raison $\frac{m-M}{m-1} =$ Const. etoit juste à la rigueur, il n'y auroit plus moyen de remedier au defaut qui resulte de la diverse refrangibilité des rayons, de quelque maniere qu'on disposeroit divers milieux transparens, et que l'intervalle entre les divers foyers tiendroit toujours un raport constant à la distance focale entiere du verre. Mais c'est precisement cette consideration, qui me fournit le plus fort argument: l'oeil me paroit une telle machine dioptrique parfaite, qui ne se ressent en aucune maniere de la diverse refrangibilité des rayons: quelque petite que soit sa distance focale, la sensibilité est si grande, que les divers foyers, s'il y en avoit, ne manqueroient pas de troubler tres considerablement la vision. Or il est bien certain, qu'un oeil bien constitué ne sent point l'effet de la diverse refrangibilité.

La structure merveilleux de l'oeil, et les diverses humeurs, dont il est composé, me confirme infiniment dans ce sentiment. Car s'il s'agissoit seulement de produire une representation sur le fond de l'oeil, une seule humeur auroit été suffisante; et le Createur n'y auroit pas surement employé plusieurs. Delà je conclud, qu'il est possible d' anéantir l'effet de la diverse refrangibilité des rayons par une juste

arrangement de plusieurs milieux transparens, donc puisque cela ne seroit pas possible, si la formule $\frac{m-M}{m-1}=$ Const. étoit vraye à la rigueur, j'en tire la consequence qu'elle n'est pas parfaitement conforme à la nature.

Mais voila une preuve directe de ma these: je conçois diverse milieuz transparens, *A, B, C, D, E, &c.* qui different entr'eux également par raport à leur densité optique: desorte que la raison de refraction de chacun dans le suivant soit le meme. Soit donc dans le passage du premier dans le second la raison de refraction pour les rayons rouges$=r:1$, et pour les violets$=v:1$; qui sera la meme dans le passage du second dans le troisieme, de celuicy dans le quatrieme, du quatrieme dans le cinquieme, et ainsi de suite. Delà il est clair, que dans le passage du premier dans le troisieme sera$=r^2:1$ pour les rayons rouges, et$=v^2:1$ pour les violets: de meme dans le passage du premier dans le quatrieme les raisons seront $r^3:1$ et $v^3:1$.

Donc si dans le passage dans un milieu quelconque la raison de refraction des rayons rouges est$=r^n:1$, celle des rayons violets sera $=v^n:1$; tout cela est parfaitement conforme aux principes du grand Newton. Posons $r^n=R$, et $v^n=V$, desorte que $R:1$, et $V:1$ expriment les raisons de refraction des rayons rouges et violets dans un passage quelconque: et ayant $n\,l\,r=l\,R$ et $n\,l\,v=l\,V$ nous aurons $l\,R:l\,r=l\,V:l\,v$, ou $\frac{l\,R}{l\,V}=\frac{l\,r}{l\,v}$. Ou bien mettes $v=r^\alpha$. et à cause de $l\,v=\alpha\,l\,r$, on aura $\frac{l\,R}{l\,V}=\frac{1}{\alpha}$, ou $l\,V=\alpha\,l\,R$, et partant $V=R^\alpha$.

Voilà donc le fondement du principe, que j'ai employé dans ma piece, qui me paroit encore inebranlable: cependant j'en soumets la decision à l'illustre Societé Royale, et à votre jugement en particulier, ayant l'honneur d'étre avec la plus parfaite consideration,

MONSIEUR,

Votre très humble

et très obéissant serviteur,

L. Euler.

Berlin,
Juin 15, 1752.

A Description of a Contrivance for measuring small Angles, *by Mr.* John Dollond; *communicated by Mr.* J. Short, F.R S.

Read May 10, 1753.

LET an object-glass of any convenient focal length (being truly ground and well centred) be divided into two equal parts or segments, by cutting it straight through the center; and let a piece of machinery be so contrived, as to hold these two segments in the same position to each other, as they stood in before they were cut asunder; and to be capable at the same time of drawing them to different distances from that position, in the manner as is represented in the figure.

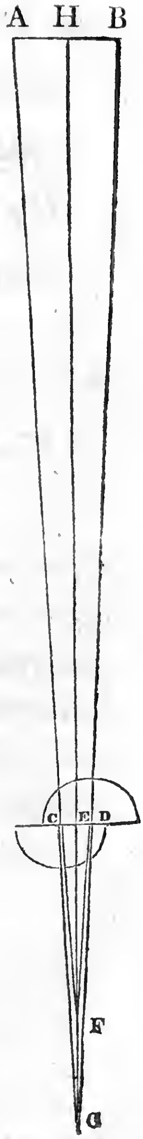
Each of these segments will form a distinct image of any object to which they are directed; differing in nothing from that, which might have been made by the whole glass before it was cut, except in brightness. And while these segments are held in their original position, the images will coincide, and become one single image as at first; but, in proportion as they are drawn off from that situation, the images will separate more or less, according to the distance they are drawn to. By this means the images of two different objects, or of different parts of the same object, not very far from each other, may

be brought to a contact or coincidence at the focus: and this coincidence may be viewed to a very great nicety with a proper eye-glass.

The measure of the angle subtended by the two objects, whose images are thus brought to a coincidence, depends upon three things: first, a careful observation of the coincidence of the images:—secondly, an exact measure of the distance, which the glasses are drawn out to from that situation, which makes the image single:—and, lastly, a true knowledge of the focal distance of the glass. How the angle is to be found from these measures, and how it may likewise be come at, by viewing two land-objects at a convenient distance, will be shewn hereafter in the explanation of the figure. It is easy to understand, in the meantime, that the angle will be measured with more accuracy, in proportion to the length of the glass, which is used for that purpose; but the difficulty of managing long telescopes is no less apparent. Therefore the most practicable method of using this micrometer to advantage, is, to apply the divided object-glass to the object end of a reflecting telescope: for, as the apertures of these sort of telescopes are large in proportion to their lengths, they will admit of very long glasses; nor will the measures be any way affected by the metals or glasses, which the reflector is composed of: and the angles will be found in the same manner, as though the images were viewed with a single eye-glass, in the manner of a common refracting astronomical telescope; but with this advantage, that, as the images will be exhibited larger and distincter by the reflecting telescope; and as every part thereof will be much more manageable than a long refracting telescope; so the contact or coincidence of the images will be more accurately observed.

It would be however unnecessary now, as well as improper, to say much about the advantages of this method above those which have hitherto been put in practice; because, as a machine is now making for this purpose, the experiments, which will shortly be tried, will be more convincing, as well as more intelligible, than any thing that might be offered at present.

Explanation of the Figure.



The two semicircles represent the two segments of the object-glass, whose centers *C* and *D* are drawn off to the distance *C D*, and the points *A* and *B* are two objects, or different parts of the same object; therefore the lines *A C G* and *B D G* represent two rays that pass through the centres or poles of the segments, and are therefore not at all refracted, but go straight through to *G*, where they intersect; and *G* being the respective focus to the distance of the objects from the glass, the two images will coincide at that point. It appears from the figure, that $AB:CD::GH:GE$; and from a common proportion in optics, $GH:GE::HE:EF$. Therefore, $AB:CD::HE:EF$; *F* being the focus of parallel rays; and consequently the angles *AEB* and *CFD* are equal. That is, the angle subtended by the distance of the centres of the segments from the distance of the focus of parallel rays is equal to the angle subtended by the distance between the objects *A* and *B* from the end of the telescope.

An Explanation of an Instrument for measuring small Angles, the first Account of which was read before the Royal Society, May 10, 1753. *By Mr.* John Dollond. *In a Letter to* James Short, M.A. *and* F.R.S.

Read April 25, 1754.

SIR,

THE account which I gave you, some time ago, of a new micrometer, was contained in as few words as possible; being rather desirous, that experiments might be made, before I said much concerning it:—but since your many repeated experiments have confirmed what was expected from it, I have endeavoured to draw up a more full account of this instrument, with demonstrations of the principles which it is founded upon, which I here send you enclosed, and which you may lay before the Royal Society, if you think proper.

I am, SIR,

Your most obedient, humble servant,

John Dollond.

Denmark Court,
April 4, 1754.

Explanation of an Instrument for Measuring Small Angles, &c.

BEFORE I enter upon particulars relating to this micrometer, it will be proper to make a few preparatory observations on the nature of spherical glasses, so far as may be necessary to render the following explanation more easily understood.

Observation I.—It is a property of all convex spherical glasses to refract the rays of light, which are transmitted through them, in such a manner, as to collect all those that proceed diverging from any one point of a luminous object, to some other point; whose distance from the glass depends chiefly on its convexity, and the distance of the object from it.

Observation II.—The point, where the rays are thus collected, may be considered as the image of that point, from which they diverge. For if we conceive several radiant points thus emitting rays, which, by the refractive quality of the glass, are made to converge to as many other points, it will be an easy matter to understand, how every part of the object will be truly represented. As this property of spherical glasses is explained and demonstrated by all the writers on optics, it being the very foundation of the science, the bare mention of it is sufficient for the present purpose.

Observation III.—It will be necessary, however, to observe farther, that the lines connecting every point in the object, with its corresponding ones in the image, do all intersect in a certain point of the axis or line passing through the poles of the glass, where its two

surfaces are parallel, and may be properly called its centre: whence it appears, that the angles subtended by the object and its image from that point, must be equal: and therefore their diameters will be in the same ratio, as their distances from that point.

Observation IV.—As the formation of the image by the glass depends entirely on the property above-mentioned, that is to say, its collecting all the light, that is incident on it, from the several points of the object into as many other points at its focus; it follows, that any segment of such a glass will also form an image equal, and every way similar, to that exhibited by the whole glass; with this difference only, that it will be so much darker, as the area of the segment is less than that of the whole glass.

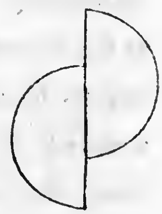
Observation V.—The axis of a spherical glass in a line connecting the centres of the spheres, to which the two surfaces are ground; and wherever this line passes through the glass, there the surfaces are parallel. But if it happens, that this line does not go through the substance of the glass, such a glass is said to have no internal centre; but it is conceived to be in its plane produced, till it meets the axis: and this imaginary point, though external to the glass, is as truly its centre, and is as fixed in its position to it, as if it were actually within its substance.

Observation VI.—If a spherical glass, having its centre or pole near its middle or centre of its circumference, should be divided by a straight line through the middle; the centre will be in one of the segments only. For how exact soever a person may be supposed to be in cutting it through the centre; yet 'tis hard to conceive, how a mathematical point should be divided in two: therefore the centre will

be internal to one of the segments, and external to the other. But if a small matter be ground away from the straight edge of each segment, both their centres will become external; and so they will more easily be brought to a coincidence.

Observation VII.—If these two segments should be held together, so as to make their centres coincide; the images, which they give of any object, will likewise coincide, and become a single one. This will be the case, when their straight edges are joined to make the glass, as it were, whole again: but let the centres be any-how separated, their images will also separate, and each segment give a separate and distinct image of any object, to which they may be exposed.

Observation VIII.—Though the centres of the segments may be drawn from their coincidence, by removing the segments in any direction whatever; yet the most convenient way for this purpose is, to slide their straight edges one along the other, till they are removed, as the figure in the margin represents them: for thus they may be moved without suffering any false light to come in between them. And by this way of removing them, the distance between their centres may be very conveniently measured; *viz.* by having a Vernier's division, commonly, though falsely, called a Nonnius's, fixed to the brass-work, that holds one segment, so as to slide along a scale on the plate, to which the other part of the glass is fitted.



Observation IX.—As the images of the same object are separated by the motion of the segments, so those of different objects, or different parts of the same object, may be made to coincide. Suppose the sun, moon, or any planet, to be the object; the two images

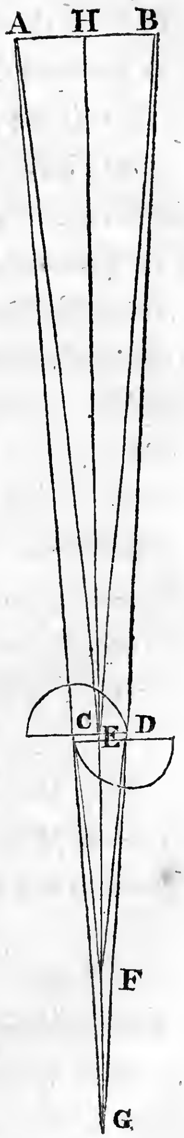
thereof may, by this contrivance, be removed, till their opposite edges are in contact: in which case, the distance between the centres of the two images will be equal to the diameter of either; and so of any other object whatever.

Observation X.—This divided glass may be used, as a micrometer, three different ways. In the first place, it may be fixed at the end of a tube, of a suitable length to its focal distance, as an object-glass; the other end of the tube having an eye-glass fitted as usual in astronomical telescopes. Secondly, it may be applied to the end of a tube much shorter than its focal distance, by having another convex glass within the tube, to shorten the focal distance of that, which is cut in two. Lastly, it may be applied to the open end of a reflecting telescope; either of the Newtonian, Gregorian, or Cassegrain construction. And though this last method is much the best, and most convenient, of the three; yet, as the first is the most natural, as well as the easiest to be understood, it will be proper to explain it fully, and to demonstrate the principles, on which this micrometer is constructed, by supposing it made use of in the first way:—which being done, the application of it to other methods will be readily understood.

Having thus, by the foregoing observations, given a general idea of the nature and effects of this divided object-glass, I shall proceed to demonstrate the principles, from whence the measures of the angles are to be obtained by this instrument; which will be done by the following propositions.

PROPOSITION I.

Suppose a divided object-glass fixed at the end of a tube, according to the first method, and the tube directed to the object intended to be measured; and suppose, likewise, the segments removed from their original position, in the manner directed under Observation VIII. *till the opposite edges of the two images are seen in contact at the focus of the eye-glass: then, I say, the angle subtended, by the distance between the centres of the segments, from the focus of the eye-glass, where the edges are seen in contact, is equal to the angle subtended by the diameter of the object from that same point.*



DEMONSTRATION.

Let the line *A B* represent the diameter of the object to be measured; and the points *C D* the centres of the two glass segments: also *G* the focus where the images of the extremities of the object are coincident. It is evident, from *Observation* III. that *A G* and *B G* are straight lines, that pass through the centres of the segments, and connect the extreme points of the object with their corresponding points in the images; and therefore, as the diameter of the object and the distance between the centres of the segments are both inscribed between these two lines, they must needs subtend the

same angle from the point where those lines meet; which is at *G*. *Q. E. D.*

The focal distance *C G*, or *D G*, is variable, according to the distance of the object from the glass: so that it decreases as the distance of the object from the glass increases; and when the object is so far off, that the focal length of the glass bears no proportion to its distance; then will it be least of all, as *C F* or *D F*; and the point *F* is called the focus of parallel rays. Any other focus, as *G*, being the focus of a near object, is called a respective focus; as it respects a particular distance: but the focus of parallel rays respects all objects that are at a very great distance; such as is that of all the heavenly bodies.

PROPOSITION II.

The distance H E *of the object from the glass is to* E F, *the focal distance of parallel rays, as the distance* H G *of the object from its image is to* E G, *the distance of the image from the glass: that is,* H E : E F :: H G : E G.

The demonstration of this proposition may be gathered from any treatise of dioptrics; it being a general rule for finding the respective focus to any given distance, when the focus of parallel rays is known.

PROPOSITION III.

The angle subtended by the diameter of the object, from the glass, is equal to that subtended, by the opening of the centres of the segments, from the focus of parallel rays. That is, the angle A E B *equal to the angle* C F D.

DEMONSTRATION.

It appears, by inspection of the figure, that $AB : CD : : HG : EG$.

And by the last proposition $HE : EF : : HG : EG$.

Then, as the two last terms of these two analogies are alike; the two first terms of one will be in the same proportion as the two first terms of the other; which gives the following proportion: $AB : CD : : HE : EF$. Whence the truth of the proposition is evident.

From this proposition it appears, that the angle subtended by the diameter of the object from the glass, is found without any regard to the distance of the object, or to the distance of the respective focus, where the image is seen; as the measure depends intirely upon the focus of parallel rays and the opening of the segments. We may likewise, from hence, derive a rule for the quantity of the angle, without considering the length of the glass. Let an object, whose diameter is known, be set up at some known distance; the angle it will subtend from the glass may then be found by trigonometry: then let it be measured by this micrometer, and the distance, between the centres of the segments, found on the scale already mentioned, will be the constant measure of the same angle, in all other cases: because the distance of the object makes no alteration in the measure of the angle, as has been demonstrated: and thus having obtained the distance between the centres of the segments, which answers to any one angle, all other distances may be computed by the rule of three.

All that has been hitherto said relates to the first method of using this micrometer; that is, by fitting it to the end of a tube suited to its focal length, and by viewing the images with a proper eye-glass, in the manner of an astronomical telescope. But the length of the

tube, in this way, would be very troublesome; and therefore it will be proper to consider other methods, for an easier management. I shall, therefore, proceed to the second method, mentioned in *Observation* X. which is, by using another object glass to shorten the focus of that which serves for the micrometer. To facilitate the understanding of this method; it will be necessary to premise the following observation.

Observation XI.—Rays of light, which are brought to such convergency as to form the image of an object, proceed, after that, diverging, in the manner they did when they issued from the object before they were transmitted through the glass; and therefore they may be again collected by another spherical glass, so as to form a second representation of the same object; which may again be repeated by a third glass, &c. So that the first image may be considered as an object to the second glass, and the second image will be an object to the third, and so on. Though these images may be very different, in respect to their magnitudes, yet they will be all similar; being true representations of the same object: this will hold good, though the second glass should be put so near the first as to receive the rays before the image is formed: for as the rays are tending to meet at a certain distance, the second will receive them in that degree of convergency, and, by an additional refraction, bring them to a nearer focus; but the image will still be similar to that which would have been made by the first glass, if the second had not been there.

Upon this principle all refracting telescopes are made; some of which are a combination of four, five, or six glasses. The first glass forms an image of the object; the second repeats the image, which it receives from the first; and so on, till the last glass brings a true

representation of the object to the eye. The same may be said of reflecting telescopes: for a spherical mirror acts in the same manner, in that respect, as a spherical glass.

Now let this be applied to the subject in hand. Suppose the focal distance of the divided object-glass to be about forty feet; and suppose the segments to be opened wide enough to bring the opposite edges of an object in contact: then let another object-glass, uncut, be fixed within the tube, of a proper degree of convexity, to shorten the focus of the other as much as may be required; suppose to twelve feet: by what has been just now observed, this glass will represent the two images in the same form which would have been exhibited by the divided glass, if this other glass had not been there. For though the images are not yet formed, when the second glass receives the rays: yet, as those rays are converging towards it, the second glass must represent those images in the same position, and form, as the tendency of the rays requires. For while the segments are fixed in their position to each other, their images will also be fixed in their position; and let them be repeated ever so many times, by refraction through spherical glasses, or by reflection from spherical mirrors, they can suffer no alteration in their position to one another. By this means, the telescope may be shortened, at pleasure, though the scale for the measure of the angles will remain the same. The only inconvenience, which the shortness of the telescope introduces, is a want of sufficient distinctness; which will so far hinder the exactness of the observation, as the contact of the edges cannot be so accurately determined, as they might be with longer telescopes.

This difficulty is intirely removed by fixing the divided glass at the

end of a reflecting telescope: for the reflections and refractions, which the rays must undergo in passing through the telescope, will no way alter the position of the images, which the rays, that have passed through the segments, are tending to: for, as has been already observed, a number of reflections and refractions may repeat the images, and alter their magnitudes; but can make no alteration in their proportions.

Therefore this way of fixing the divided glass to a reflecting telescope, which was the third method proposed, is, by far, the best; as such telescopes of moderate and manageable lengths, when well made, are capable of magnifying considerably, and shewing objects to great advantage. This micrometer being applicable to the reflecting telescope, with so much certainty, is no inconsiderable advantage: for any one will easily understand, that, to measure the diameter of a planet exactly, it is necessary, that the planet be magnified, and shown distinctly, which could not be obtained, in the common way, without very great lengths; such as rendered it very difficult, not to say impracticable, to take exact measures. Besides, the common micrometer is limited, in this respect, upon another account; *viz.* because the diameter of the planet cannot be measured, without having the whole planet within the field of the telescope, which confines the magnifying power within very narrow bounds; whereas, by this method, nothing more is required, than to see the contact of the edges, which allows the magnifying power to be increased at pleasure.

In the common micrometer, the object is to be taken between two wires, so that the contact of its edges with those wires cannot be observed at one view; and the least motion of the telescope, whilst

the observer is turning his eye from one wire to the other, must oblige him to repeat the observation; whereas, by this method, the contact of the edges of the images is not at all affected by the motion of the telescope. Whence the comparison of this micrometer with the common sort, in this respect, stands thus: the one requires great steadiness in the telescope, but yet it is applicable to none, but such as are very difficult to keep steady; the other does not require such steadiness, though it is applicable to short telescopes, which are easily managed.

These advantages not only add to the certainty of the observation, but assist vastly in the expedition; for an observer may make twenty observations, in this way, where he could scarcely, with much fatigue, be sure of one with the common micrometer. Expedition in making observations, must be allowed a very great advantage, in this climate, where the uncertainty of the weather renders astronomical observations so precarious, that no opportunities, even the most transient, should be let slip. An instance of this was given to the Royal Society, in an account of the eclipse of the sun last October.

As the motion of the telescope gives the observer no great inconvenience, in this method; neither does the motion of the object at all disturb his observation (I mean such a motion, as that of the heavens is.) This gives him leave to take the diameter of a planet, in any direction; or the distance between two stars or planets, let their situation be how it will; in which respect the common micrometer is absolutely defective; as it can give no angles, but such as are perpendicular to the line of their motion; though the diameters of the planets, in other directions, are very much wanted; it being highly

probable, from the laws of motion, and what we see in Jupiter, that such planets, as revolve round their axes, have their polar diameters shorter than their equatorial ones.

The distances of Jupiter's satellites from one another, or from Jupiter's body, cannot be measured, with any certainty, in the common way, as their position is always very far from being at right angles with the line of their motion: neither can the moon's diameter, which must be taken from horn to horn, scarce ever be obtained that way, because it very rarely happens, that the diameter, to be measured, lies at right angles to the line of her motion. The same may be said of the distance between two stars. But this micrometer gives angles, in every direction, with equal ease and certainty; the observation being also finished in an instant, without any trouble or fatigue to the observer. For as there are no wires made use of, this way, in the field of the telescope, so the observer has no concern about any illumination. The largeness of the scale deserves also to be taken notice of, as it may, in this micrometer, be increased almost at pleasure, according as the smallness of the object requires. Another inconvenience attending the common micrometer is, the variation of the scale, according to the distance of the object. As the telescope must be lengthened, or drawn out farther, for short distances; the scale, which depends upon that length, is thereby increased; which renders the measure of the angle very uncertain: whereas, in this micrometer, the scale is the same at all distances; so that the angle may be measured with the utmost certainty, without any regard to the distance of the object.

Upon the whole, it may be concluded, that this micrometer is a complete instrument in its kind; having many advantages above the common sort, without any of their disadvantages: and there is no doubt, but, when brought into practice, it will tend much to the advancement of astronomy.

An Account of some Experiments concerning the different Refrangibility of Light. By Mr. John Dollond. *With a Letter from* James Short, M.A. F.R.S. *Acad. Reg. Suec. Soc.*

To the Rev. Dr. Birch, *Secret.* R.S.

Read June 8, 1758.

DEAR SIR,

I HAVE received the enclosed paper from Mr. Dollond, which he desires may be laid before the Royal Society. It contains the theory of correcting the errors arising from the different refrangibility of the rays of light in the object-glasses of refracting telescopes; and I have found, upon examination, that telescopes made according to this theory are intirely free from colours, and are as distinct as reflecting telescopes.

I am, DEAR SIR,

Your most obedient humble servant,

James Short.

Surrey Street,
June 8, 1758.

Experiments concerning the different Refrangibility of Light, &c.

It is well known, that a ray of light, refracted by passing through mediums of different densities, is at the same time proportionally divided or spread into a number of parts, commonly called homogeneal rays, each of a different colour; and that these, after refraction, proceed diverging; a proof, that they are differently refracted, and that light consists of parts that differ in degrees of refrangibility.

Every ray of light passing from a rarer into a denser medium, is refracted towards the perpendicular; but from a denser into a rarer one, from the perpendicular; and the sines of the angles of incidence and refraction are in a given ratio. But light consisting of parts, which are differently refrangible, each part of an original or compound ray has a ratio peculiar to itself; and therefore the more a heterogeneous ray is refracted, the more will the colours diverge, since the ratios of the sines of the homogeneal rays are constant; and equal refractions produce equal divergencies.

That this is the case when light is refracted by one given medium only, as suppose any particular sort of glass, is out of all dispute, being indeed self-evident; but that the divergency of the colours will be the same under equal refractions, whatsoever mediums the light may be refracted by, though generally supposed, does not appear quite so clearly.

However, as no medium is known, which will refract light without diverging the colours, and as difference of refrangibility seems thence

to be a property inherent in light itself, opticians have, upon that consideration, concluded, that equal refractions must produce equal divergencies in every sort of medium: whence it should also follow, that equal and contrary refractions must not only destroy each other, but that the divergency of the colours from one refraction would likewise be corrected by the other; and there could be no possibility of producing any such thing as refraction, which would not be affected by the different refrangibility of light; or, in other words, that however a ray of light might be refracted backwards and forwards by different mediums, as water, glass, *&c.* provided it was so done, that the emergent ray should be parallel to the incident one, it would ever after be white; and conversely, if it should come out inclined to the incident, it would diverge, and ever after be coloured. From whence it was natural to infer, that all spherical object-glasses of telescopes must be equally affected by the different refrangibility of light, in proportion to their apertures, whatever material they may be formed of.

But it seems worthy of consideration, that notwithstanding this notion has been generally adopted as an incontestable truth, yet it does not seem to have been hitherto so confirmed by evident experiments, as the nature of so important a matter justly demands; and this it was that determined me to attempt putting the thing to issue by the following experiment.

I cemented together two plates of parallel glass at their edges, so as to form a prismatic or wedge-like vessel, when stopped at the ends or bases; and its edge being turned downwards, I placed therein a glass prism with one of its edges upwards, and filled up the vacancy

with clear water: thus the refraction of the prism was contrived to be contrary to that of the water, so that a ray of light transmitted through both these refracting mediums would be refracted by the difference only between the two refractions. Wherefore, as I found the water to refract more or less than the glass prism, I diminished or increased the angle between the glass plates, till I found the two contrary refractions to be equal; which I discovered by viewing an object through this double prism; which, when it appeared neither raised nor depressed, I was satisfied, that the refractions were equal, and that the emergent rays were parallel to the incident.

Now, according to the prevailing opinion, the object should have appeared through this double prism quite of its natural colour; for if the difference of refrangibility had been equal in the two equal refractions, they would have rectified each other: but the experiment fully proved the fallacy of this received opinion, by showing the divergency of the light by the prism to be almost double of that by the water; for the object, though not at all refracted, was yet as much infected with prismatic colours, as if it had been seen through a glass wedge only, whose refracting angle was near 30 degrees.

N.B. This experiment will be readily perceived to be the same as that which Sir Isaac Newton mentions*; but how it comes to differ so very remarkably in the result, I shall not take upon me to account for; but will only add, that I used all possible pre-

* Book I. Part ii. Prop. 3. Experiment viii. of his Optics.

caution and care in the process, and that I keep the apparatus by me to evince the truth of what I write, whenever I may be properly required so to do.

I plainly saw then, that if the refracting angle of the water-vessel could have admitted of a sufficient increase, the divergency of the coloured rays would have been greatly diminished, or entirely rectified; and there would have been a very great refraction without colour, as now I had a great discolouring without refraction: but the inconveniency of so large an angle, as that of the vessel must have been, to bring the light to an equal divergency with that of the glass prism, whose angle was about 60 degrees, made it necessary to try some experiments of the same kind, by smaller angles.

I ground a wedge of common plate glass to an angle of somewhat less than 9 degrees, which refracted the mean rays about 5 degrees. I then made a wedge-like vessel, as in the former experiment, and filling it with water, managed it so, that it refracted equally with the glass wedge; or, in other words, the difference of their refractions was nothing, and objects viewed through them appeared neither raised nor depressed. This was done with an intent to observe the same thing over again in these small angles, which I had seen in the prism: and it appeared indeed the same in proportion, or as near as I could judge; for notwithstanding the refractions were here also equal, yet the divergency of the colours by the glass was vastly greater than that by the water; for objects seen by these two refractions were very much discoloured. Now this was a demonstration, that the divergency of the light, by the different refrangibility, was

far from being equal in these two refractions. I also saw, from the position of the colours, that the excess of divergency was in the glass; so that I increased the angle of the water-wedge, by different trials, till the divergency of the light by the water was equal to that by the glass; that is, till the object, though considerably refracted, by the excess of the refraction of the water, appeared nevertheless quite free from any colours proceeding from the different refrangibility of light; and, as near as I could then measure, the refraction by the water was about $\frac{5}{4}$ of that by the glass. Indeed I was not very exact in taking the measures, because my business was not at that time about the proportions, so much as to show, that the divergency of the colours, by different substances, was by no means in proportion to the refractions; and that there was a possibility of refraction without any divergency of the light at all.

Having, about the beginning of the year 1757, tried these experiments, I soon after set about grinding telescopic object-glasses upon the new principles of refractions, which I had gathered from them; which object-glasses were compounded of two spherical glasses with water between them. These glasses I had the satisfaction to find, as I had expected, free from the errors arising from the different refrangibility of light: for the refractions, by which the rays were brought to a focus, were everywhere the differences between two contrary refractions, in the same manner, and in the same proportions, as in the experiment with the wedges.

However, the images formed at the foci of these object-glasses were still very far from being so distinct as might have been expected from the removal of so great a disturbance; and yet it was not very diffi-

cult to guess at the reason, when I considered, that the radii of the spherical surfaces of those glasses were required to be so short, in order to make the refractions in the required proportions, that they must produce aberrations, or errors, in the image, as great, or greater, than those from the different refrangibility of light. And therefore, seeing no method of getting over that difficulty, I gave up all hopes of succeeding in that way.

And yet, as these experiments clearly proved, that different substances diverged the light very differently, in proportion to the refraction; I began to suspect, that such a variety might possibly be found in different sorts of glass, especially as experience had already shown, that some made much better object-glasses, in the usual way, than others: and as no satisfactory cause had as yet been assigned for such difference, there was great reason to presume, that it might be owing to the different divergency of the light by their refractions.

Wherefore, the next business to be undertaken, was to grind wedges of different kinds of glass, and apply them together, so that the refractions might be made in contrary directions, in order to discover, as in the foregoing experiments, whether the refraction and divergency of the colours would vanish together. But a considerable time elapsed before I could set about that work; for though I was determined to try it at my leisure, for satisfying my own curiosity, yet I did not expect to meet with a difference sufficient to give room for any great improvement of telescopes; so that it was not till the latter end of the year that I undertook it, when my first trials convinced me, that this business really deserved my utmost attention and application.

I discovered a difference, far beyond my hopes, in the refractive qualities of different kinds of glass, with respect to their divergency of colours. The yellow or straw-coloured foreign sort, commonly called Venice glass, and the English crown glass, are very nearly alike in that respect, though in general the crown glass seems to diverge the light rather the least of the two. The common plate glass made in England diverges more; and the white crystal or flint English glass, as it is called, most of all.

It was not now my business to examine into the particular qualities of every kind of glass that I could come at, much less to amuse myself with conjectures about the cause, but to fix upon such two sorts whose difference was the greatest; which I soon found to be the crown, and the white flint or crystal. I therefore ground a wedge of white flint of about 25 degrees, and another of crown of about 29 degrees, which refracted nearly alike; but their divergency of the colours was very different. I then ground several others of crown to different angles, till I got one, which was equal, with respect to the divergency of the light, to that in the white flint: for when they were put together, so as to refract in contrary directions, the refracted light was intirely free from colour. Then measuring the refractions of each wedge, I found that of the white glass to be to that of the crown nearly as 2 to 3; and this proportion would hold very nearly in all small angles. Wherefore any two wedges made in this proportion, and applied together, so as to refract in a contrary direction, would refract the light without any difference of refrangibility.

To make therefore two spherical glasses, that shall refract the light in contrary directions, it is easy to understand, that one must be concave, and the other convex; and as the rays are to converge to a real focus, the excess of refraction must evidently be in the convex; and as the convex is to refract most, it appears from the experiment, that it must be made with crown glass, and the concave with white flint glass.

And further, as the refractions of spherical glasses are in an inverse ratio of their focal distances; it follows, that the focal distances of the two glasses should be inversely as the ratios of the refractions of the wedges: for being thus proportioned, every ray of light that passes through this combined glass, at whatever distance it may pass from its axis, will constantly be refracted, by the difference between two contrary refractions, in the proportion required; and therefore the different refrangibility of the light will be intirely removed.

Having thus got rid of the principal cause of the imperfection of refracting telescopes, there seemed to be nothing more to do, but to go to work upon this principle: but I had not made many attempts, before I found, that the removal of one impediment had introduced another equally detrimental (the same as I had before found in two glasses with water between them): for the two glasses, that were to be combined together, were the segments of very deep spheres; and therefore the aberrations from the spherical surfaces became very considerable, and greatly disturbed the distinctness of the image. Though this appeared at first a very great difficulty, yet I was not long without hopes of a remedy: for considering

the surfaces of spherical glasses admit of great variations, though the focal distance be limited, and that by these variations their aberrations may be made more or less, almost at pleasure, I plainly saw the possibility of making the aberrations of any two glasses equal; and as in this case the refractions of the two glasses were contrary to each other, their aberrations, being equal, would intirely vanish.

And thus, at last, I obtained a perfect theory for making object-glasses, to the apertures of which I could scarcely conceive any limits: for if the practice could come up to the theory, they must certainly admit of very extensive ones, and of course bear very great magnifying powers.

But the difficulties attending the practice are very considerable. In the first place, the focal distances, as well as the particular surfaces, must be very nicely proportioned to the densities or refracting powers of the glasses; which are very apt to vary in the same sort of glass made at different times. Secondly, the centres of the two glasses must be placed truly on the common axis of the telescope, otherwise the desired effect will be in a great measure destroyed. Add to these, that there are four surfaces to be wrought perfectly spherical; and any person, but moderately practised in optical operations, will allow, that there must be the greatest accuracy throughout the whole work.

Notwithstanding so many difficulties, as I have enumerated, I have, after numerous trials, and a resolute perseverance, brought the matter at last to such an issue, that I can construct refracting telescopes, with

such apertures and magnifying powers, under limited lengths, as, in the opinion of the best and undeniable judges, who have experienced them, far exceed any thing that has been hitherto produced, as representing objects with great distinctness, and in their true colours.

John Dollond.

Peter Dollond Esq.r

Engraved by J. Thomson from an original Painting by J. Heppner R. A.

Some Account of the Discovery, made by the late Mr. John Dollond, F.R S. *which led to the grand Improvement of* Refracting Telescopes, *in Order to correct some Misrepresentations, in Foreign Publications, of that Discovery: with an Attempt to account for the* Mistake *in an Experiment made by Sir* Isaac Newton; *on which Experiment, the Improvement of the* Refracting Telescope *intirely depended. By* Peter Dollond, *Member of the American Philosophical Society at Philadelphia.*

ADVERTISEMENT.

MY intention in writing the following paper was, to correct several false representations, relating to the invention of the achromatic refracting telescope, and to secure to my late father, Mr. John Dollond, as well as to this country, the honour of so valuable a discovery.

With this view, the paper was presented to the Royal Society, by the Rev. Dr. Maskelyne, Astronomer Royal, in expectation of its being published in the Philosophical Transactions. It was read at a meeting of the Society on the 21st of May, 1789; but afterwards, contrary to my expectation, it was resolved, in a council of the Society, that the paper should *not* be printed in their Transactions; I therefore take this method of submitting it to the public; as I humbly conceive, it relates to a subject of a sufficient degree of importance to claim their attention.

Peter Dollond.

St. Paul's Church-yard,
Sept. 1, 1789.

Some Account of the Improvement in Refracting Telescopes, &c.

THE correction of any inaccuracies or false representations in the history of science is certainly of some consequence to the public, and deserves the attention of the Royal Society; particularly so, when such false representation tends to deprive any one of that praise, to which he may be justly entitled, by having contributed towards the advancement of science; even though it may be in things of little moment. Then certainly it must be much more so, when it relates to matters of great importance; such as was the discovery which brought forward the grand improvement of the refracting telescope.

I was led to these reflections, by having seen some accounts of that discovery in different publications, which were related in a manner that lessened the merit of my late father John Dollond, and gave it to others, who never thought themselves in any manner entitled to claim with him, or ever appeared to be inclined so to do. Their own characters were too exalted in science to need any additional merit of any discovery, to which they had not an undoubted right.

The celebrated M. Euler, of Berlin, and M. Klinginstierna, professor of mathematics at Upsal, in Sweden, are the persons alluded to. These gentlemen have been mentioned by different foreign authors, who have had occasion to give accounts of the improvement of the refracting telescope, as being the discoverers of the PRINCIPLE on which that improvement was founded; and nothing has been left to Dollond, but the credit of being the first who put the

same into practice; whereby he has been deprived of the honour which is justly due to his memory, for having made so useful a discovery.

In order to set this matter in a proper light, I shall mention so much from Sir Isaac Newton's Optics, as is necessary for the purpose; and then endeavour to prove, that what was attempted by Euler and Klinginstierna was not done from any knowledge of the principle on which the improvement was founded; but that Dollond was actually the discoverer of that principle, as well as the person who first put the same in practice.

When Newton had made his great discovery of the different refrangibility of light, he fully explained *that* to be the cause of the imperfection of refracting telescopes, and that it was not occasioned by the spherical figures of the glasses, as has been the generally received opinion. But as mathematicians had made many attempts to correct the errors arising from spherical figures, by giving the glasses figures from the conic sections, he took that opportunity of mentioning an ingenious thought of his own, of composing the object-glasses of two glasses with water between them; by which means he says, that the spherical figures of the glasses might have been corrected, and telescopes brought to a sufficient perfection, had it not been for the different refrangibility of the several sorts of rays.—*Newton's Optics, 3d. Edition, p.* 90.

Newton having completed the principal experiments relating to the different refrangibility of light, and having determined the proportions of the sines of incidence to the sines of refractions in the different coloured rays, as given by his glass prisms, proceeds to try the eighth

experiment of the second part of the first book of his Optics, to discover their proportions in different refracting mediums. This experiment he tried, by placing a prism of glass in a prismatic vessel of water. Refracting the light through these different mediums, he found that light, as often as by contrary refractions it is so corrected that it emergeth in lines parallel to those in which it was incident, continues ever after to be white; but if the emergent rays be inclined to the incident, the light will become coloured.—*Newton's Optics, p.* 112.

The conclusion drawn from this experiment was, that the divergency of the different coloured rays was constantly in a given proportion to the mean refraction in all sorts of refracting mediums. This was the principle established by the Newtonian experiment, and was doubted by no one, until the beginning of the year 1757; when Dollond tried the same experiment as above related, and found the result to be very different; for the light after being refracted in contrary directions through the glass and water prisms, if the emergent rays were parallel to the incident rays, they were found to be considerably coloured; from whence it followed, that the dissipation of the different coloured rays was not in the same proportion to the mean refraction in water as in glass. And further experiments proved, that there was also a very considerable difference of the same nature to be found in different kinds of glass.—*See this Appendix, p.* 50.

This was the new principle, which brought forward the improvement of refracting telescopes; a principle so contrary to the generally received opinion, that Euler had much difficulty to prevail on himself to believe what was told him by his friends on that subject;

as appears by his own papers published in the Memoirs of the Royal Academy at Berlin. For he first supposes the goodness of Dollond's telescopes to be owing to the greenish colour of the crown-glass, which did not transmit all the red rays; he afterwards endeavours to account for it from the construction of the eye-glasses; and at last declares it to be very extraordinary, that the English optician should have made such an improvement, by reasoning, as it were, in a manner quite contrary to the nature of things; for so indeed the new discovered principle appeared to him. Notwithstanding these declarations of Euler, which were published in the year 1762, M. De la Lande, in the second volume of his Astronomy, p. 837, published in the year 1764, says, that Euler, in 1747, endeavoured to correct the different refrangibility of light, by a method which Newton pointed out for correcting the errors of the spherical figures of the glasses; which was, by two lenses with water between them, as recited above: and he says, that Dollond tried to confute Euler, who had demonstrated an error in Newton's theory of colours; but the dispute having given occasion to Dollond to examine the thing more narrowly, he afterwards acknowledged the error of Newton, and in the year 1759 he found out a method of making achromatic telescopes that succeeded very well.

Now this account of De la Lande's is by no means the true state of the facts, as appears by the Letters which passed between Euler and Dollond, *see the former part of this Appendix, pp.* 21—32; for though Euler argues from his hypothesis, that the result of Newton's experiment could not be exactly as he relates it, yet he does not pretend to controvert any of Newton's laws of refraction, as being con-

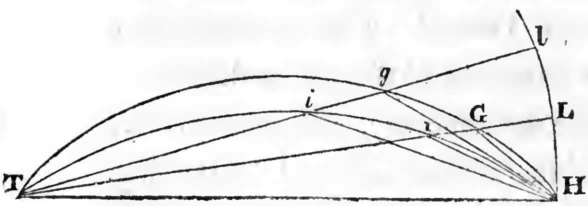
trary to experiment, but believed, that the divergency of the different coloured rays differed scarce sensibly from bearing a given proportion to the mean refraction, in all sorts of refracting mediums; by which it appears, that the error afterwards discovered by Dollond was not even suspected by Euler; therefore that part of De la Lande's account cannot be true; for Dollond could not be said to acknowledge an error, supposed to be discovered by Euler in Newton's theory of colours, by having actually discovered one himself of a different nature. The true state of the fact is, that in 1747 Euler endeavoured to correct the errors arising from the different refrangibility of light in object-glasses, by a method which was not founded on any experiment, but on an hypothesis, which did not appear to be on a true principle, so that the attempts which were made to put this method in practice did not succeed: this was certainly the case; for after M. Clairaut had examined the controversy between Euler and Dollond, he pronounced, that what Euler had done appeared to be more ingenious than useful.

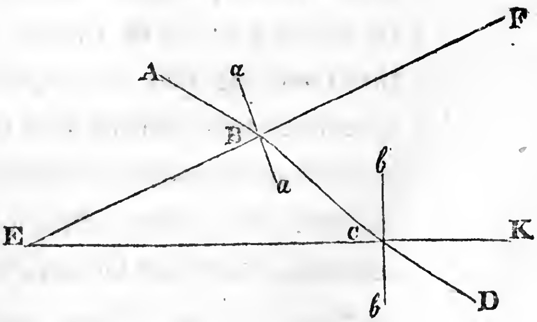
Euler indeed says, that the structure of the eye gave him the greatest reason to suppose, that the different refrangibility might be corrected by several refractions through different kinds of mediums; for which purpose he thought the eye to be so constructed. But this reasoning had no weight with Dollond, as he perceived and mentioned to his friends, that the refractions of the eye, at the several surfaces and humours, are all made the same way, and consequently, for want of contrary refractions, the colours produced by the first refractions could not be taken away. How this can subsist with the perfection of our vision, has been ingeniously explained by the

Astronomer Royal, in an account which he proposes to lay before the Society.—*See p. 78 of this Appendix.*

Klinginstierna has likewise been considered as a party concerned in the improvement of the refracting telescope; though De la Lande does not mention his name, yet some others do. This has been occasioned by his having, in the year 1755, considered the controversy between Euler and Dollond, and having formed a theorem of his own, by which he was also induced to believe, that the result of Newton's experiment could not be as he had related it; except when the angles of the refracting mediums were small. This he communicated to Dollond, in a letter to his friend Mr. Mallet, who was then in England. As this theorem has never been published in English, I shall give a copy of it here, as taken by my father from Klinginstierna's letter to Mallet, that mathematicians may judge of the truth of the deductions.

" *Remarks on the Law of Refraction of Rays of Light of different Kinds, through different Mediums. See Newton's Optics, Book* I. *Part* ii. *Prop.* 3. *Exper.* viii.





" Upon any right line, as TH, let there be drawn two arches TIH, TGH, and let a right line TIG be drawn intersecting the arches in I and G. Join IH and GH; let F E K be a transparent wedge, having its acute angle FEK equal to the angle IHG; let the two faces of this wedge be contiguous to two different transparent mediums; and let the ratio of refraction out of the medium, that joins the surface EF into the wedge, be as the ratio of TH to TI, and the ratio of refraction out of the wedge into the medium joining the surface EK as the ratio of TG to TH.

" Now if AB represents a ray of light entering into the wedge, and the angle AB *a* is made equal to HIG, then will the angle CB *a* be equal to the angle THI. Ang. BC *b*=ang. THG and DC *b*= HGL; so that the incident ray AB will be parallel to the emergent ray CD, which has been twice refracted.

" Now if the incident ray is compounded of divers simple rays, each of which, after two refractions, should emerge parallel to the common incident ray, the refractions of each will be represented by so many right lines T*ig* joining H*i*, H*g*, the same as before.

" According to Newton's law of refraction quoted above TH—TI should be in a constant ratio to TH—TG; that is, if an arch of a circle is described on the centre T with the interval TH, meeting the lines TIG, T*ig* in L and *l*; then by that supposition LI should be to LG as *li* to *lg*. But these proportions will not hold, unless L and *l* were in an arch described on the chord TH, but they are in an arch whose centre is T.

" Therefore Newton's law of refraction does not seem to follow

clearly from his 8th experiment, which our wedge with two contiguous mediums refers to.

"If we should suppose such a law of refraction as we find necessary to bring out every simple ray parallel to the incident, after two refractions through this wedge FEK, it can be demonstrated, that the same law will not have the same effect in another wedge of a different angle, but for every different angle there will be a different law required.

"Whence it seems to follow, that there must be some mistake in this experiment of Newton's, which he himself gives as an universal one, for it does not seem likely that the law which really obtains in nature should depend upon a greater or less angle of a wedge.

"Nevertheless it must be observed, that the less the refractions are, the nigher will the Newtonian law be to that which is required for producing a perfect parallellism of the emergent rays to the common incident ray; for in this case LI to LG will be very nearly in a given ratio. It does not seem that the aberration of the rays in object-glasses, proceeding from the different refrangibility, can be corrected by any refractions, which is what Mr. Newton plainly insists upon. However, this whole affair deserves to be more accurately examined by experiments."

I shall here only remark, that it appears by this copy of a letter from Klinginstierna, that the supposed error in the result of Newton's experiment, which he thus labours to demonstrate, is the same as before attempted to be ascertained by Euler, and not the error which was afterwards discovered by Dollond.

The account given by De la Lande, of the improvement of the refracting telescope, was copied by most foreign writers on the subject, with little variation, except in giving sometimes a little more of the honour to Euler, and also making mention of Klinginstierna.

But in the Eulogy on Euler, written and published by M. N. Fuss, professor of mathematics at St. Petersburg, in the year 1783, p. 41 and 42, he gives the whole of the discovery to Euler, except "that Dollond is allowed to have found out two sorts of glass, which crowned at last, in 1757, the happy conjecture of Euler, by the invention of achromatic telescopes, which formed a new epoch in astronomy and dioptrics." As this account is the most curious of any I have found, I shall here give it at length, and contrast it with what Euler says himself on the subject, in a paper read before the Royal Academy of Sciences at Berlin, in 1764, and published in the volume of their Memoirs for 1766, p. 119.

Mr. Fuss says, "The examination of the Newtonian theory had given Euler an opportunity of investigating the different refrangibility of light, and the bad effects which the dispersion of the colours produced in refracting telescopes, which had been almost intirely abandoned upon account of this defect. The consideration of the wonderful structure of the eye made him suppose, that a certain combination of different transparent bodies could remedy this inconvenience. He proposed for this purpose, in the year 1747, object-glasses composed of two glasses, the cavity between which could be filled with water.

"His opinion was attacked by the famous English artist, Dollond, who opposed to him the authority of Newton: M. Euler soon shewed

him the error of his principles. Some experiments made upon meniscuses, the cavities of which were filled with different liquids, confirmed him in his opinion: and Mr. Dollond, who had in the mean time discovered two sorts of glass, which were proper for examining it further, crowned at last, in 1757, the happy conjecture of M. Euler, by the invention of achromatic telescopes, which formed an epoch in astronomy and dioptrics.

" The success of Mr. Dollond, who availed himself, with so much advantage, of a discovery, which he had at first attacked as contrary to experiment, induced M. Euler to extend his researches further upon the subject of dioptric instruments, &c."

I shall now subjoin a translation of M. Euler's paper, read at the Academy of Sciences at Berlin in the year 1764.

" Although I have already frequently discussed this subject, I see myself again obliged to resume it, in consequence of the astonishing discoveries which have been lately made upon the nature of glass, and its different kinds. I am not ashamed frankly to avow, that the first accounts, which were published of it, appeared so suspicious, and even so contrary to the best established principles, that I could not prevail upon myself to give credit to them. That there should be two kinds of glass, in which the refraction of the mean rays is nearly equal, whilst that of the extremes differs most enormously, appeared to me to shock good sense; and perhaps I should never have submitted to the proofs, which Mr. Dollond produced to support this strange phenomenon, if Mr. Clairaut, who must at first have been equally surprized at it, had not most positively assured me, that Dollond's experiments were but too well founded. But at length the experi-

ments made at Petersburg by M. Zeiher have effectually succeeded in removing my prejudice; that ingenious philosopher having incontestably proved that it is the lead, which is used in some compositions of glass, that produces in it that strange quality of augmenting the dispersion of the extreme rays, without changing sensibly the refraction of the mean; and by increasing the quantity of lead in the composition of glass, he has been enabled to make glass, which produces a much greater dispersion of the rays than the flint-glass of Dollond.

"Now I must intirely renounce this principle, which until now has appeared so well-founded, that the dispersion of the extreme rays depends solely upon the refraction of the mean rays; and I am obliged to acknowledge, that the dispersion depends principally upon the quality of the glass, without the mean refraction thereof being sensibly affected thereby."

As it appears by the above paper, that Euler was at last so convinced of the truth of the discovery made by Dollond, as to renounce his favourite hypothesis, it must be inferred, that the account given of this matter by Fuss is very far from being the true state of the facts, and indeed so much so, as to be very inexcusable, even in an eulogy.

There is another publication of a later date, which I shall take the liberty of mentioning, "*Extracts of the Observations made at the Royal Observatory at Paris, in the year* 1787, *by Count Cassini.*" In page 106 he gives an account of the improvement of the refracting telescope, by way of prelude to his describing a method proposed by M. l'Abbé Rochon, of putting fluids, and also a kind of mastic, between the glasses of achromatic object-glasses, as a good method of

mending bad glasses, or, as he calls it, a method to correct the *non-sphericity* of the glasses; which he mentions as being similar to that ingenious idea proposed by Newton, for correcting the errors of the spherical figures of object-glasses. The account he gives of the improvement of the refracting telescope is as follows. He says, "It was the celebrated Euler who first proposed to correct the errors arising from the different refrangibility, by using different refracting mediums, such as water and glass. The late Mr. Dollond having availed himself of and realized this ingenious idea, has a just right to *partake* of the glory."—By these publications it seems, that Dollond, who explained the fallacy of Euler's hypothesis, who afterwards discovered the true principle, on which the different refrangibility of light could be corrected, and he, who put the same in practice, so much to the benefit of science, is only to be allowed to *partake* of the glory, and that with Euler, who never himself thought he had the least right to claim any part of the discovery with Dollond, as most fully appears by the paper above recited from the Berlin Memoirs.

I can account for these false representations no other way than by supposing, that those who wrote them have not taken sufficient pains to inform themselves of the true history of the discovery; for I would not wish to attribute what they have said to any partiality; and I am induced to hope, that when the state of facts which I have here adduced shall be candidly considered, that they will retract their declarations, and acknowledge, that Dollond was the sole discoverer of the principle which led to the improvement of refracting telescopes.

I now come to a more agreeable part of this paper, which is, to endeavour to reconcile the different results of the 8th experiment of

the second part of the 1st Book of Newton's Optics, as related by himself, and as it was found by Dollond, when he tried the same experiment, in the year 1757. Newton says, that light, as often as by contrary refractions it is so corrected, that it emergeth in lines parallel to the incident, continues ever after to be white. Now Dollond says when he tried the same experiment, and made the mean refraction of the water equal to that of the glass prism, so that the light emerged in lines parallel to the incident, he found the divergency of the light by the glass prism to be nearly double to what it was by the water prism. The light appeared to be so evidently coloured, that it was directly said by some persons, that if Newton had actually tried the experiment, he must have perceived it to have been so. Yet who could for a moment doubt the veracity of such a character?—therefore different conjectures were made by different persons. Mr. Murdoch in particular gave a paper to the Royal Society in defence of Newton; but it was such as very little tended to clear up the matter. *Philosophical Transactions, vol.* liii. *p.* 192.—Some have supposed that Newton made use of water strongly impregnated with *Saccharum Saturni,* because he mentions sometimes using such water, to increase the refraction, when he used water prisms instead of glass prisms. *Newton's Optics, p.* 62.—And others have supposed, that he tried the experiment with so strong a persuasion in his own mind, that the divergency of the colours was always in the same proportion to the mean refraction, in all sorts of refracting mediums, that he did not attend so much to that experiment as he ought to have done, or as he usually did. None of these suppositions having appeared at all satisfactory, I have therefore endeavoured to find out the true cause of the difference,

and thereby shew, how the experiment may be made to agree with Newton's description of it, and to get rid of those doubts, which have hitherto remained to be cleared up.

It is well known, that in Newton's time the English were not the most famous for making optical instruments:—telescopes, opera-glasses, &c. were imported from Italy in great numbers, and particularly from Venice; where was manufactured a kind of glass which was much more proper for optical purposes than any made in England at that time. The glass made at Venice was nearly of the same refractive quality as our crown glass, but of a much better colour, being sufficiently clear and transparent for the purpose of prisms. It is probable that Newton's prisms were made with this kind of glass; and it appears to be the more so, because he mentions the specific gravity of common glass to be to water as 2.58 to 1. *Newton's Opt. p.* 247, which nearly answers to the specific gravity we find the Venetian glass generally to have. Having a very thick plate of this kind of glass, which was presented to me about twenty-five years ago by the late professor Allemand, of Leyden, and which he then informed me had been made many years, I cut a piece from this plate of glass to form a prism, which I conceived would be similar to those made use of by Newton himself. I have tried the Newtonian experiment with this prism, and find it answers so nearly to what Newton relates, that the difference which remains may very easily be supposed to arise from any little difference, which may and does often happen in the same kind of glass made at the same place at different times. Now the glass prism made use of by Dollond to try the same experiment was made of English flint glass, the specific gravity of which I have never known

to be less than 3, 22. This difference in the densities of the prisms used by Newton and Dollond was sufficient to cause all the difference which appeared to the two experimenters in trying the same experiment.

From this it appears, that Newton was accurate in this experiment as in all others, and that his not having discovered that, which was discovered by Dollond so many years afterwards, was owing intirely to accident; for if his prism had been made of glass of a greater or less density, he would certainly have then made the discovery, and refracting telescopes would not have remained so long in their original imperfect state.

An Attempt to explain a Difficulty in the Theory of Vision, depending on the different Refrangibility of Light. By the Rev. Nevil Maskelyne, D.D. F.R S. *and Astronomer Royal.*

Read June 18, 1789.

THE ideas of sight are so striking and beautiful, that we are apt to consider them as perfectly distinct. The celebrated Euler, taking this for granted, has supposed, in the Memoirs of the Royal Academy of Sciences at Berlin for 1747, that the several humours of the human eye were contrived in such a manner as to prevent the latitude of focus arising from the different refrangibility of light, and considers this as a new reason for admiring the structure of the eye; for that a single transparent medium, of a proper figure, would have been sufficient to represent images of outward objects in an imperfect manner; but to make the organ of sight absolutely complete, it was necessary it should be composed of several transparent mediums,

properly figured, and fitted together agreeable to the rules of the sublimest geometry, in order to obviate the effect of the different refrangibility of light in disturbing the distinctness of the image; and hence he concludes, that it is possible to dispose four refracting surfaces, in such a manner as to bring all sorts of rays to one focus, at whatever distance the object be placed. He then assumes a certain hypothesis of refraction of the differently refrangible rays, and builds thereon an ingenious theory of an achromatic object-glass, composed of two meniscus glasses with water between them, with the help of an analytical calculation, simple and elegant, as his usually are.

He has not, however, demonstrated the necessary existence of his hypothesis, his arguments for which are more metaphysical than geometrical; and, as it was founded on no experiments, so those made since have shewn its fallacy, and that it does not obtain in nature. Moreover, which is rather extraordinary, it does not account, according to his own ideas, for the very phenomenon which first suggested it to him, namely, the great distinctness of the human vision, as was observed to me, many years ago, by the late Mr. John Dollond, F.R.S. to whom we are so much obliged for the invention of the achromatic telescope;* for the refractions at the several humours of the eye

* As a misstatement of this fact has been made by both *Paley* and *Priestley*, we shall quote their own words for the satisfaction of the reader.—" At last it came into the mind of a sagacious optician, to inquire how this matter was managed in the eye; in which there was exactly the same difficulty to contend with, as in the telescope. His observation taught him, that, in the eye, the evil was cured by combining together lenses composed of different substances, i. e. of substances which possessed different

being all made one way, the colours produced by the first refraction will be increased at the two subsequent ones instead of being corrected, whether we make use of Newton's or Euler's law of refraction of the differently refrangible rays.

Thus Euler produced an hypothetical principle, neither fit for rendering a telescope achromatic, nor to account for the distinctness of the human vision; and the difficulty of reconciling that distinctness with the principle of the different refrangibility of light discovered by Sir Isaac Newton remains in full force.

In order to go to the bottom of this difficulty, as the best probable means of obviating it, I have calculated the refractions of the mean, most, and least refrangible rays at the several humours of the eye, and thence inferred the diffusion of the rays, proceeding from a point in an object, at their falling upon the retina, and the external angle which such coloured image of a point upon the retina corresponds to.

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refracting powers. Our artist borrowed from thence his hint; and produced a correction of the defect, by imitating, in glasses made from different materials, the effects of the different humours through which the rays of light pass before they reach the bottom of the eye."—*See Paley, p.* 23. " M. Euler did not pretend to controvert the experiments of Newton; but he said that they were not contrary to his hypothesis, but in so small a degree as might be neglected, and asserted that, if they were admitted in all their extent, it would be impossible to correct the difference of refrangibility occasioned by the transmission of the rays from one medium into another of different density; a correction which, he thought, was very possible, since he supposed it to be actually effected in the structure of the eye, which he thought was made to consist of different mediums for that very purpose. To this kind of reasoning Mr. Dollond made no reply; but by appealing to the experiments of Newton, and the great circumspection with which it was known that he conducted all his inquiries."—*See Priestley, p.* 458.

I took the dimensions of the eye from M. Petit, as related by Dr. Jurin; and, the specific gravities of the aqueous and vitreous humours having been found to be nearly the same with that of water, and the refraction of the vitreous humour of an ox's eye having been found by Mr. Hawksbee to be the same as that of water, and the ratio of refraction out of air into the crystalline humour of an ox's eye having been found by the same accurate experimenter to be as 1 to ,68327, I took the refraction of the mean refrangible rays out of air into the aqueous or vitreous humour, the same as into water, as 1 to ,74853, or 1,33595 to 1; and out of air into the crystalline humour as 1 to ,68327, or 1,46355 to 1. Hence I find, according to Sir Isaac Newton's two theorems, related at Part II. of Book I. of Optics, p. 113, that the ratio of refraction of the most, mean, and least refrangible rays at the cornea should be as 1 to ,74512, ,74853 and ,75197; at the fore-surface of the crystalline as 1 to ,91173, ,91282, and ,91392; and at the hinder-surface of the crystalline as 1 to 1,09681, 1,09550, and 1,09420.

Now, taking with Dr. Jurin 15 inches for the distance at which the generality of eyes in their mean state see with most distinctness, I find the rays from a point of an object so situate will be collected into three several foci, *viz.* the most, mean, and least refrangible rays at the respective distances behind the crystalline, ,5930, ,6034, and ,6141 of an inch, the focus of the most refrangible rays being ,0211 inch short of the focus of the least refrangible ones.

Moreover, assuming the diameter of the pencil of rays at the cornea, proceeding from the object at 15 inches distance, to be $\frac{1}{5}$th of an inch in a strong light, which is a large allowance for it, the semi-

angle of the pencil of mean refrangible rays at their concourse upon the retina will be 7° 12′, whose tangent to the radius unity, or ,1264 multiplied into ,0211 inch, the interval of the foci of the extreme refrangible rays, gives ,002667 inch for the diffusion of the different coloured rays, or the diameter of the indistinct circle upon the retina. Now, I find, that the diameter of the image of an object upon the retina is to the object as ,6055 inch to the distance of the object from the centre of curvature of the cornea; or the size of the image is the same as would be formed by a very thin convex lens, whose focal distance is, 6055 inch, and consequently a line in an object which subtends an angle of 1′ at the centre of the cornea will be represented on the retina by a line of $\frac{1}{5678}$th inch. Hence the diameter of the indistinct circle on the retina before found, ,002667 will answer to an external angle of $,002667 \times 5678' = 15'\ 8''$, or every point in an object should appear to subtend an angle of about 15′, on account of the different refrangibility of the rays of light.

I shall now endeavour to shew that this angle of ocular aberration is compatible with the distinctness of our vision. This aberration is of the same kind as that which we experience in the common refracting telescope. Now, by computation from the tabular apertures and magnifying powers of such telescopes, it is certain that they admit of an angular indistinctness at the eye or no less than 57′; therefore the ocular aberration is near four times less than in a common refracting telescope, and consequently the real indistinctness, being as the square of the angular aberration, will be 14 or 15 times less in the eye than in a common refracting telescope, which may be easily allowed to be imperceptible.

Moreover, Sir Isaac Newton has observed, with respect to the like difficulty of accounting for the distinctness with which refracting telescopes represent objects, that the erring rays are not scattered uniformly over the circle of dissipation in the focus of the object-glass, but collected infinitely more densely in the centre than in any other part of the circle, and in the way from the centre to the circumference grow continually rarer and rarer, so as at the circumference to become infinitely rare; and by reason of their rarity are not strong enough to be visible, unless in the centre and very near it.

He farther observes, that the most luminous of the prismatic colours are the yellow and orange, which affect the sense more strongly than all the rest together; and next to these in strength are the red and green; and that the blue, indigo, and violet, compared with these, are much darker and fainter, and compared with the other stronger colours, little to be regarded; and that therefore the images of the objects are to be placed not in the focus of the mean refrangible rays, which are in the confine of green and blue, but in the middle of the orange and yellow, there where the colour is most luminous, that which is in the brightest yellow, that yellow which inclines more to orange than to green.

From all these considerations, and by an elaborate calculation, he infers, that though the whole breadth of the image of a lucid point be $\frac{1}{55}$th of the diameter of the aperture of the object-glass, yet the sensible image of the same is scarce broader than a circle whose diameter is $\frac{1}{250}$th part of the diameter of the aperture of the object-glass of a good telescope; and hence he accounts for the apparent diameters of the fixed stars as observed with telescopes by astronomers, although in reality they are but points.

The like reasoning is applicable to the circle of dissipation on the retina of the human eye; and therefore we may lessen the angular aberration, before computed at 15′, in the ratio of 250 to 55, which will reduce it to 3′ 18″.

This reduced angle of aberration may perhaps be double the apparent diameter of the brightest fixed stars to an eye disposed for seeing most distinctly by parallel rays; or, if short-sighted, assisted by a proper concave lens; which may be thought a sufficient approximation in an explication grounded on a dissipation of rays, to which a precise limit cannot be assigned, on account of the continual increase of density from the circumference to the centre. Certainly some such angle of aberration is necessary to account for the stars appearing under any sensible angle to such an eye; and if we were, without reason, to suppose the images on the retina to be perfect, we should be put to a much greater difficulty to account for the fixed stars appearing otherwise than as points, than we have now been to account for the actual distinctness of our sight.

The less apparent diameter of the smaller fixed stars agrees also with this theory; for the less luminous the circle of dissipation is, the nearer we must look towards its centre to find rays sufficiently dense to move the sense. From Sir Isaac Newton's geometrical account of the relative density of the rays in the circle of dissipation, given in his system of the world, it may be inferred, that the apparent diameters of the fixed stars, as depending on this cause, are nearly as their whole quantity of light.

In farther elucidation of this subject let me add my own experiment. When I look at the brighter fixed stars, at considerable

elevations, through a concave glass fitted, as I am short-sighted, to shew them with most distinctness, they appear to me without scintillation, and as a small round circle of fire of a sensible magnitude. If I look at them without the concave glass, or with one not suited to my eye, they appear to cast out rays of a determinate figure, not exactly the same in both eyes, somewhat like branches of trees (which doubtless arise from something in the construction of the eye) and to scintillate a little, if the air be not very clear. To see day objects with most distinctness, I require a less concave lens by one degree than for seeing the stars best by night, the cause of which seems to be, that the bottom of the eye being illuminated by the day objects, and thereby rendered a light ground, obscures the fainter colours blue indigo and violet in the circle of dissipation, and therefore the best image of the object will be found in the focus of the bright yellow rays, and not in that of the mean refrangible ones, or the dark green, agreeable to Newton's remark, and consequently nearer the retina of a short-sighted person; but the parts of the retina surrounding the circle of dissipation of a star being in the dark, the fainter colours, blue, indigo, and violet, will have some share in forming the image, and consequently the focus will be shorter.

The apparent diameter of the stars here accounted for is different from that explained by Dr. Jurin, in his Essay on Distinct and Indistinct Vision, arising from the natural constitution of the generality of eyes to see objects most distinct at moderate distances, and few being capable of altering their conformation enough to see distant objects, and among them the celestial ones, with equal distinctness.

But the cause of error, which I have pointed out, will affect all eyes, even those which are adapted to distant objects.

If this attempt to shew the compatibility of the actual distinctness of our sight with the different refrangibility of light shall be admitted as just and convincing, we shall have fresh reason to admire the wisdom of the creator in so adapting the aperture of the pupil and the different refrangibility of light to each other, as to render the picture of objects upon the retina relatively, though not absolutely, perfect, and fitted for every useful purpose; " where," to borrow the words of our religious and oratorical philosopher Derham, " all the glories of the heavens and earth are brought and exquisitely pictured."

Nor does it appear, that any material advantage would have been obtained, if the image of objects on the retina had been made absolutely perfect, unless the acuteness of the optic nerve should have been increased at the same time; as the *minimum visibile* depends no less on that circumstance than the other. But that the sensibility of the optic nerve could not have been much increased beyond what it is, without great inconvenience to us, may be easily conceived, if we only consider the forcible impression made on our eye by a bright sky, or even the day objects illuminated by a strong sun. Hence we may conclude, that such an alteration would have rendered our sight painful instead of pleasant, and noxious instead of useful. We might indeed have been enabled to see more in the starry heavens with the naked eye, but it must have been at the expence of our daily labours and occupations, the immediate and necessary employment of man.

I shall only mention farther, and obviate an objection to the dif-

fusion of the rays upon the retina by the different refrangibility of light. It may be said, that the ocular aberration, being a separate cause from any effect of the telescope, should subsist equally when we observe a star through a telescope as when we look at it with the naked eye; and that therefore the fixed stars could not appear so small as they have been found to do through the best telescopes, and particularly by Dr. Herschel with his excellent ones. To this I answer, that the ocular aberration, which is proportional to the diameter of the pupil when we use the naked eye, is proportional to the diameter of the pencil of rays at the eye when we look through a telescope, which being many times less than that of the pupil itself, the ocular aberration will be diminished in proportion, and become insensible.

An Account of an Improvement made by Mr. Peter Dollond *in his New Telescopes. In a Letter to* James Short, M.A. F.R.S. *with a Letter of Mr.* Short's *to the Rev.* Thomas Birch, D.D. *Secret.* R.S.

DEAR SIR,

I HAVE sent you inclosed, a letter which I received this morning from Mr. Dollond, concerning an improvement which he has made in his new telescopes. He, some months ago, sent me a telescope, in this new way, of $3\frac{1}{2}$ feet focal length, with an aperture of $3\frac{3}{4}$ inches; I examined it, and I approved of it; I have tried it with a magnifying power of 150 times, and I found the image distinct, bright, and free from colours.

You may, if you please, lay Mr. Dollond's letter before the Royal Society.

I am, DEAR SIR,

Your most obedient and humble servant,

James Short.

Surrey Street,
February 7, 1765.

Mr. Dollond's Letter to Mr. Short.

Read February 7, 1765.

SIR,

I TAKE the liberty of sending you the following short account of an improvement I have lately made in the compound object glasses of refracting telescopes.

The dissipation of the rays of light may be perfectly corrected in object glasses, by combining mediums of different refractive qualities; and the errors or aberrations of the spherical surfaces may be corrected by the contrary refractions of two lenses, made of the different mediums; yet as the excess of refraction is in the convex lens, and though the surfaces of the concave lens may be so proportioned as to aberrate exactly equal to the convex lens, near the axis; yet as the refractions of the two lenses are not equal, the equality of the aberrations cannot be continued to any great distance from the axis.

In the year 1758, when my father had constructed some object glasses for telescopes in this manner, viz. with one convex lens of crown glass, and one concave lens of white flint glass; he attempted

to make short object glasses to be used with concave eye glasses, in the same manner; but as the field of view, in using a concave eye glass depends on the aperture of the object glass, the limits of the aperture were found to be too small: this led my father to consider that if the refraction of the crown glass (in which the excess was) should be divided by means of having two lenses made of crown glass instead of one, the aberration would thereby be decreased, and the apertures might then be larger: this was tried with success in those object glasses, when concave eye glasses were used, and these have been ever since made in this manner: some trials were likewise made, at the same time, to enlarge the apertures of longer object glasses, where convex eye glasses were used, by the same method; but these not succeeding, in the same manner, the method of making them with one lens of crown glass, and one of white flint glass, was continued.

As I could not see any good reason why the method, which was practised with so much success, when concave eye glasses were used, should not do with convex ones; I determined to try some further experiments in that way. After a few trials, I found it might be done; and in a short time I finished an object glass of 5 feet focal length, with an aperture of $3\frac{3}{4}$ inches, composed of two convex lenses of crown glass, and one concave of white flint glass.

Thinking that the apertures might be yet admitted larger; I attempted to make one of $3\frac{1}{2}$ feet focal length, with the same aperture of $3\frac{3}{4}$ inches, which I have now completed, and am ready to show the same to the Royal Society, if desired.

The difficulty of procuring good glass of so large a diameter, and

of the thickness required, added to the great exactness of the surfaces, in order to correct the aberration in such large apertures, has prevented me from attempting to extend them any farther in that length.

I am, SIR,

Your most obedient,

and most humble servant,

Peter Dollond.

A Letter from Mr. Peter Dollond, *to* Nevil Maskelyne, F.R.S. *& Astronomer Royal; describing some Additions and Alterations made to* Hadley's Quadrant, *to render it more serviceable at Sea.*

Read March 29, 1772.

REVEREND SIR,

THE particular attention which you have always shown to any improvement tending to the advantage of astronomy or navigation, makes me take the liberty to trouble you with an account of some additions and alterations which I have lately made to the Hadley's quadrant.

The general use of this instrument at sea is so well known, that no mention need be made of the importance of any improvements in the construction, that may render the observations more exact, and occasion more frequent opportunities of making them.

The glasses of the Hadley's quadrant should have their two surfaces perfect planes, and perfectly parallel to each other. From several years practice in grinding these glasses, I have found out methods of making them to great exactness; but the advantage, that should arise from the goodness of the glasses, has often times been defeated by the index glass being bent by the brass frame that contains it: to prevent this, I have contrived the frame, so that the glass lies on three points, and the part that presses against the front of the glass has also three points exactly opposite to the former. These points are made to confine the glass by three screws at the back, that act exactly opposite to the points between which the glass is placed. This little contrivance may be of some use; but the principal improvements are in the methods of adjusting the glasses, particularly for the back observation.

The method hitherto practised for adjusting that part of the instrument, by means of the opposite horizons at sea, has been attended with so many difficulties that it has scarcely ever been used; for so little dependance could be placed on the observations taken this way, that the best Hadley's sextants made for the purposes of observing the distances of the moon from the sun or fixed stars, have been always made without the horizon glass for the back observation; for want of which, many valuable observations of the sun and moon have been lost, when their distance has exceeded 120 degrees.

To make the adjustment of the back observation easy and exact, I have applied an index to the back horizon glass, by which it may be moved into a parallel position to the index glass, in order to give it the two adjustments, in the same manner as the fore horizon glass is

adjusted. Then, by moving the index to which the back horizon glass is fixed, exactly 90 degrees (which is known by the divisions made for that purpose) the glass will be thereby set at right angles to the index glass, and consequently will be properly adjusted for use, and the observations may be made with the same accuracy by this, as by the fore observation.

To adjust the horizon glasses in the perpendicular position to the plane of the instrument, I have contrived to move each of them by a single screw, that goes through the frame of the quadrant, and is turned by means of a milled head at the back, which may be done by the observer while he is looking at the object.

To these improvements, Sir, I have added your method of placing darkening glasses behind the horizon glasses, which you have been so kind as to give me liberty to apply to my instruments. These glasses, which serve for darkening the object seen by direct vision, in adjusting the instrument by the sun or moon, I have placed in such a manner as to be turned behind the fore horizon glass, or behind the back horizon glass, that they may be used with either; there are three of these glasses of different degrees of darkness; the lightest or palest I do imagine will be of use in taking the sun's altitude when the horizon appears glaring, which I believe often happens by the reflection of the sea.

If these additions and alterations should be thought to be real improvements, which I cannot doubt, Sir, if they are honoured with your approbation, I hope they may serve in conjunction with those improvements you have made yourself in respect to the obviating any possible errors in the parallelism of the planes of the index glass, and

in regard to the adjustment of the telescope parallel to the plane of the quadrant, to extend the use of this most valuable nautical instrument, and to add to the exactness of the celestial observations taken with it to determine the longitude at sea. But of these particulars I need say no more, since you are, without doubt, in every respect, the properest person to give an account of them.

I am, SIR,

Your most obedient,

humble servant,

Peter Dollond.

London,
February 25, 1772.

Remarks on the Hadley's Quadrant, *tending principally to remove the Difficulties which have hitherto attended the Use of the Back-observation, and to obviate the Errors that might arise from a Want of Parallelism in the two Surfaces of the Index-Glass. By* Nevil Maskelyne, F.R.S. *Astronomer Royal.*

Read May 28, 1772.

THE back-observation with Hadley's quadrant being founded on the same principles, and in theory, equally perfect with the fore observation, and being at the same time necessary to extend the use of the instrument up to 180 degrees (it being impracticable to measure angles with any convenience beyond 120 degrees with the fore-observation) it may seem surprizing that it hath not been brought equally into general use, more especially since the method of finding the longitude by observations of the moon, has been practised at sea for some years past; since this method would receive considerable

advantage from the use of the back-observation in taking distances of the sun and moon between the first and last quarter, could such observations be as much depended upon as the fore-observation. The causes of this seem to have been principally these two, the difficulty of adjusting the back-horizon-glass, and the want of a method of directing the sight parallel to the plane of the quadrant. The back-horizon-glass, like the fore-one, requires two adjustments:—the first, or common one, disposes it at right angles to the index glass, when the index stands at (0) upon the arch; which is usually performed by setting (0) of the index of the arch of the quadrant by double the dip of the horizon of the sea, and then holding the quadrant vertical with the arch downwards, and turning the back-horizon-glass about, by means of its lever or perpetual screw, till the reflected back-horizon appears to coincide with the fore-horizon seen directly. But this operation is so difficult in practice with the back-horizon-glass wholly silvered, except a small transparent slit in the middle, as it has been usually made, that few (if any) persons have ever received proper satisfaction from it. If the back-horizon-glass was silvered in every respect like the fore-horizon-glass (which it ought to be) the upper part being left unsilvered, and a telescope was applied to it, perhaps this adjustment might be rendered somewhat easier and more exact; but it could not even thus be made so exact as the adjustment of the fore-horizon-glass may, by making use of the sun's limbs.

The second adjustment of the back-horizon-glass, in the common construction of the quadrant, is still more troublesome, since it cannot be executed without setting the index 90 degrees off the arch, in order to place the index-glass parallel to the back-horizon-glass; when

this adjustment may be performed in the same manner as the corresponding adjustment of the fore-horizon-glass. But the bending of the index, that follows the setting it off the arch, is a very disagreeable circumstance, having a tendency, especially on board of ship, to expose both the index and centre work to damage; and may even, without extraordinary precautions taken by the instrument maker in placing the plane of the index-glass exactly according to the length of the index, disturb its perpendicularity to the plane of the quadrant: on these accounts it would be much better if this adjustment of the back-horizon-glass could be performed, like those of the fore-horizon-glass, with the index remaining upon the arch of the quadrant. Fortunately, this *desideratum* has been lately effected by an ingenious contrivance invented by Mr. Dollond, which he has given an account of in a letter addressed to me*, which I have presented to this Society, by means of an additional index applied to the back-horizon-glass; whereby both the adjustments may be made by the same observations and with nearly the same exactness as those of the fore-horizon-glass:—for a further knowledge of which see the account itself.

Besides the difficulty of adjusting the back-horizon-glass, the want of a method of directing the line of sight parallel to the plane of the quadrant has proved also a considerable obstacle to the use of the back-observation: this will easily appear from the following proposition, that the error of the angle measured arising from any small deviation of the visual ray from a parallelism to the plane of the quad-

* See page 92 for the Letter alluded to.

rant, is to twice an arch equal to the verse-sine of the deviation, as the tangent of half the angle measured by the quadrant is to radius, very nearly. Thus a deviation of 1° in the line of sight, will produce an error of about 1′ in measuring an angle of 90°, whether by the fore or back-observation; but the same deviation will produce an error of 4′ in measuring an angle of 150°, of 6′ in taking an angle of 160°, and 12′ in taking an angle of 170°. Hence a pretty exact adjustment of the line of sight, or axis of the telescope, is requisite in measuring large angles, such as those are taken by the back-observation: and therefore a director of the sight ought by no means to be omitted in the construction of the instrument (as it commonly has been since Mr. Hadley's time, though recommended by him), except a telescope be made use of, which, if rightly placed, answers the same purpose better, especially in observing the distance of the moon from the sun between the first and last quarter. The director of the sight may be placed exact enough by construction; but the telescope cannot, and Mr. Hadley, not having been aware of the importance of an exact position of it, has accordingly given no directions for the placing of it. I shall therefore endeavour to supply this defect in the following remarks.

In the first place, I would by all means recommend an adjusting piece to be applied to the telescope, whereby its axis may be brought parallel to the plane of the quadrant: in the next place, the back-horizon-glass ought to be silvered in the same manner as the fore-horizon-glass: and thirdly, two thick silver wires should be placed within the eye-tube in the focus of the eye-glass parallel to one another, and to the plane of the quadrant. If they were put at such

a distance as to divide the diameter of the field of view into three equal parts, it might be as convenient as any other interval. In this manner wires were placed in the telescope by Mr. Hadley, as appears by his account of the instrument in *Philosophical Transactions*, No. 420. These wires are to be adjusted parallel to the plane of the quadrant, by turning the eye-tube round about which contains the wires, till they appear parallel to the plane of the quadrant. The axis of the telescope, by which is meant the line joining the centre of the object-glass and the middle point between the two wires, is to be adjusted parallel to the plane of the quadrant by either of the two following methods.

Method I.—When the distance of the moon from the sun is greater than 90 degrees, by giving a sweep with the quadrant and moving the index, bring the nearest limbs to touch one another at the wire nearest the plane of the quadrant. Then, the index remaining unmoved, make the like observation at the wire farthest from the plane of the quadrant; and note whether the nearest limbs are in contact as they were at the other wire: if they are, the axis of the telescope is parallel to the plane of the quadrant: but if they are not, it is inclined to the same, and must be corrected as follows. If the nearest limbs of the sun and moon seem to lap over one another at the wire farthest from the plane of the quadrant, the object end of the telescope is inclined from the plane of the quadrant, and must be altered by the adjustment made for that purpose: but, if the nearest limbs of the sun and moon do not come to touch one another at the wire farthest from the plane of the quadrant, the object end of the telescope is inclined towards the plane of the quadrant, and must be altered by the adjustment accordingly. Let these operations be re-

peated until the observation is the same at both the parallel wires, and the axis of the telescope will be adjusted parallel to the plane of the quadrant. In like manner, the axis of the telescope may be also adjusted parallel to the plane of the quadrant for the fore-observation.

Method II.—Set the index to (0) and hold the plane of the quadrant parallel to the horizon of the sea, with the divided arch upwards, the two wires being parallel to, and including both the direct fore-horizon, and the reflected back-horizon, between them. Raise or lower the plane of the quadrant until the direct and reflected horizons coincide together: if the coincidence happens in the middle between the two wires, or rather, to be more exact, above the middle by such a part of the field of view as answers to the number of minutes in the depression of the horizon (which may be easily estimated if the angular interval of the wires be first found by experiment, in manner hereafter mentioned) the axis of the telescope is parallel to the plane of the quadrant; but if it does not, the line of sight is inclined to the plane of the quadrant, and must be corrected as follows. If the direct and reflected horizons, when they coincide, appear higher above the middle between the wires, than what the quantity of the depression of the horizon amounts to, the object end of the telescope is inclined from the plane of the quadrant, and must be altered by the adjustment made for that purpose; but if the two horizons appear to coincide in a lower part of the field of the telescope, the object end of the telescope is inclined towards the plane of the quadrant, and must be altered by the adjustment accordingly. Repeat these operations till the two horizons appear to coincide above the middle between

the two wires, by the quantity of the depression of the horizon, and the axis of the telescope will be adjusted parallel to the plane of the quadrant. In order to find the angular interval between the wires, hold the quadrant perpendicular to the horizon, as in observing altitudes; and turn about the eye-tube with the wires until they are parallel to, and include the direct fore-horizon and reflected back-horizon between them. Move the index from (0) along the divided arch, at the same time raising or lowering the telescope by the motion of the quadrant until the direct horizon appears to coincide with the upper wire, and the reflected back-horizon with the lower wire; the number of degrees and minutes shown upon the arch, increased by double the depression of the horizon, will be the angular interval of the wires; its proportion to the depression of the horizon will be therefore known; and hence the space in the field of the telescope answering to the depression of the horizon, may be easily estimated near enough for adjusting the axis of the telescope in the manner before mentioned. The first of the two methods here given for adjusting the position of the telescope will probably be found most convenient; and the greater the distance of the sun and moon is, the more nearly may the adjustment be made, because the same deviation of the axis of the telescope will cause a greater error.

The telescope should be fixed by the instrument-maker so as to command a full field of view when the instrument is placed at 90° if the instrument be an octant, or 120° if it be a sextant; because the index-glass then stands more oblique with respect to the incident and reflected rays, and consequently the field of view of the telescope, as far as it depends upon the index-glass, will be more contracted

than in any other position of the index: but if there is a fair field of view in this case, there necessarily must be so in every other position of the index.

The two parallel wires will be very useful on many occasions, as well in the fore as the back-observation. In taking the altitude of the sun, moon, or star, direct the sight towards the part of the horizon underneath, or opposite to the object, according as you intend to observe by the fore or back-observation, and hold the quadrant that the wires may constantly appear perpendicular to the horizon, and move the index till you see the object come down towards the horizon in the fore-observation, or up to it in the back-observation, and turn the instrument in order to bring the object between the wires; then move the index till the sun or moon's limb, or the star touch the horizon. The nearer the object is brought to an imaginary line in the middle between the wires (it is indifferent what part of the line it is brought to) and the truer the wires are kept perpendicular to the horizon, the more exact will the observation be. In the fore-observation, the object appears in its real position; but in the back-observation, the object being brought through the zenith to the horizon, the real upper-limb will appear the lowest; and the contrary. Either limb of the sun may be used in either observation; but it will be most convenient in general to make the sun appear against the sky, and not against the sea; and then the objects appearing inverted through the telescope, the sun will appear lowest, and the horizon highest. The observed altitude is to be corrected for dip, refraction, and sun's semi-diameter, as usual.

In taking the distance of the nearest limbs of the sun and moon, whether by the fore or back-observation, having first set the index to the distance nearly, by the help of the Nautical Almanac, and brought the moon to appear anywhere on or near the diameter of the field of view of the telescope, which bisects the interval between the wires, give a sweep to the quadrant, and the sun and moon will pass by one another; if in this motion the nearest limbs, at their nearest approach, just come to touch one another, without lapping over, on or near any part of the diameter of the field of the telescope which bisects the interval between the wires, the index is rightly set; but if the nearest limbs either do not come to meet, or lap over one another, alter the index, and repeat the observation till the nearest limbs come to touch one another properly. This method of observing will be found much more easy and expeditious than without the wires, since in that case it would be necessary to make the limbs touch very near the centre of the telescope, but here it is only necessary to make them do so anywhere on or near the diameter of the field of the telescope which bisects the interval between the two wires.

The same method may be used in taking the moon's distance from a fixed star.

It may not be amiss here to make some remarks on the rules that have been usually given for observing the sun's altitude, both with the fore and back-observation, which have all been defective, and to point out the proper directions to be followed, when a telescope is not used with two parallel wires to direct the quadrant perpendicular to the horizon, and to shew the principles on which these directions are founded.

Observers are commonly told, that in making the fore-observation they should move the index to bring the sun down to the part of the horizon directly beneath them, and turn the quadrant about upon the axis of vision; and when the sun touches the horizon at the lowest part of the arch described by them, the quadrant will shew the altitude above the visible horizon. I allow that this rule would be true, if a person could by sight certainly know the part of the horizon exactly beneath the sun; but, as this is impossible, the precept is incomplete. Moreover, in taking the sun's altitude in or near the zenith, this rule intirely fails, and the best observers advise to hold the quadrant vertical, and turn one's self about upon the heel, stopping when the sun glides along the horizon without cutting it: and it is certain that this is a good rule in this case, and capable with care of answering the intended purpose. We have thus two rules for the same thing, which is a proof that neither of them is an universal one, or sufficient in all cases alone.

In taking the back-observation, observers have been advised either to turn the quadrant about upon the axis of vision, or, holding the quadrant upright, to turn themselves about upon the heel, indifferently. The true state of the case is this; that, in taking the sun's altitude, whether by the fore or back-observation, these two methods must be combined together; that is to say, the observer must turn the quadrant about upon the axis of vision, and at the same time turn himself about upon his heel, so as to keep the sun always in that part of the horizon-glass which is at the same distance as the eye from the plane of the quadrant: for, unless the caution of observing the objects in the proper part of the horizon-glass be attended to, it is evident the

angles measured cannot be true ones. In this way the reflected sun will describe an arch of a parallel circle round the true sun, whose convex side will be downwards in the fore-observation, and upwards in the back-observation, and consequently, when, by moving the index, the lowest point of the arch in the fore-observation, or the uppermost point of the arch in the back-observation, is made to touch the horizon, the quadrant will stand in a vertical plane, and the altitude above the visible horizon will be properly observed.

The reason of these operations may be thus explained:—the image of the sun being always kept in the axis of vision, the index will always show on the quadrant the distance between the sun and any object seen directly which its image appears to touch; therefore, as long as the index remains unmoved, the image of the sun will describe an arch everywhere equidistant from the sun in the heavens, and consequently a parallel circle about the sun, as a pole; such a translation of the sun's image can only be produced by the quadrant being turned about upon a line drawn from the eye to the sun, as an axis; a motion of rotation upon this line may be resolved into two, one upon the axis of vision, and the other upon a line on the quadrant perpendicular to the axis of vision; and consequently a proper combination of these two motions will keep the image of the sun constantly in the axis of vision, and cause both jointly to run over a parallel circle about the sun in the heavens; but when the quadrant is vertical, a line thereon perpendicular to the axis of vision becomes a vertical axis; and, as a small motion of the quadrant is all that is wanted, it will never differ much in practice from a vertical axis; therefore the observer, by properly combining and proportioning two

motions, one of the quadrant upon the axis of vision, and the other of himself upon his heel, keeping himself upright (which gives the quadrant a motion upon a vertical axis) will cause the image of the sun to describe a small arch of a parallel circle about the sun in the heavens, without departing considerably from the axis of vision.

If it should be asked, why the observer should be directed to perform two motions rather than the single one equivalent to them on a line drawn from the eye to the sun as an axis, I answer, that we are not capable, while looking towards the horizon, of judging how to turn the quadrant about upon the elevated line going to the sun as an axis, by any other means than by combining the two motions above-mentioned, so as to keep the sun's image always in the proper part of the horizon-glass. When the sun is near the horizon, the line going from the eye to the sun will not be far removed from the axis of vision; and consequently the principal motion of the quadrant will be performed on the axis of vision, and the part of the motion made on the vertical axis will be but small. On the contrary, when the sun is near the zenith, the line going to the sun is not far removed from a vertical line, and consequently the principal motion of the quadrant will be performed on a vertical axis, by the observer's turning himself about, and the part of the motion made on the axis of vision will be but small. In intermediate altitudes of the sun, the motions of the quadrant on the axis of vision and on a vertical axis will be more equally divided. Hence appears the reason of the method used by the best observers in taking the sun's altitude when near the zenith by holding the quadrant vertical and turning about upon the heel, and the

defects of the rules that have been commonly given for observing altitudes in other cases.

As it may conduce to the setting this matter in a still clearer light, I shall here describe in order the several motions that will be given to the reflected image, by turning the quadrant about upon the axis of vision, a vertical axis, or the line drawn from the eye to the sun, successively.

I. If the quadrant is turned about upon the axis of vision, the same being directed to the point of the horizon exactly beneath or opposite the sun, the image of the sun will move from right to left, or from left to right, across the horizon-glass, the same way as the arch of the quadrant is carried, both in the fore and back-observations, with a velocity which is to the angular velocity of the quadrant as the sine of the sun's altitude to the radius, describing an arch convex downwards in both cases; and when the motion of the sun in this arch is parallel to the horizon, the quadrant is held truly perpendicular to the horizon, and consequently in a proper position for taking the sun's altitude. But, if the axis of vision be directed to, and turned round a point in the horizon beside the vertical circle passing through the sun, the sun's image, when its motion is parallel to the horizon, will be neither in the axis of vision nor the sun's vertical, but between both; at the same time, the plane of the quadrant will not be vertical, and the altitude found by bringing the sun's image to touch the horizon will not be the true altitude.

II. If the quadrant be held perpendicular to the horizon, and turned

about upon a vertical axis, or one nearly so, the sun will describe an arch convex downwards in the fore-observation, and upwards in the back-observation, the motion of the sun being the same way as the axis of vision is carried in both cases, and being to the angular motion of the quadrant, as the verse-sine of the sun's altitude to the radius in the fore-observation, but as the verse-sine of the supplement of the sun's altitude to 180° to the radius in the back-observation. The sun therefore will move slower than the axis of vision in the fore-observation, and consequently will be left behind, with respect to the axis of vision, or seem to move backwards; and the sun will move quicker than the axis of vision in the back-observation, or will seem to get before it. When the motion of the sun in this arch is parallel to the horizon, the plane of the quadrant coincides with the vertical circle passing through the sun, and consequently the quadrant is in a proper position for taking the sun's altitude. But if the quadrant be held a little deviating from the perpendicular position to the horizon, and turned about upon an axis, either vertical or only nearly so, the arch described by the sun apparently will cut the horizon, but will never move parallel to it, and consequently the quadrant will not be brought into a proper position for observing the sun's altitude.

III. If the quadrant be turned on the line going to the sun as an axis, the reflected sun will be kept constantly in the axis of vision, and will describe an arch of a parallel circle about the real sun, with a velocity which is to the angular motion of the quadrant, as the sine of the sun's altitude is to the radius; and when the

motion of the reflected sun is parallel to the horizon, the quadrant is vertical.

Hence naturally arise the three methods of taking an altitude, which have been mentioned before. In the first, the axis of vision is supposed always directed to one and the same part of the horizon, namely, that which is in the sun's vertical. In the second, the observer is required to hold the quadrant truly vertical, and to turn himself upon a vertical axis; but it is evident neither of these motions can be accurately performed. In the third method, the observer is only required to move both himself and the quadrant, so as to keep the sun always in or near the axis of vision, which may be performed very well, because the axis of vision is a visible and certain direction for it. One exception, however, should be made to this general rule, namely, in taking the sun's altitude when very low, by the back-observation: in which case it will be best to use the second method, or else to hold the quadrant perpendicular by judgment; which will be much facilitated by using a telescope containing wires in its focus parallel to the plane of the quadrant, as described in p. 103 *of this Appendix*: for, in this case, the perpendicular position of the quadrant cannot be attained so near by the method of turning the quadrant on a line going to the sun as an axis, as it can by any other method.

It remains to treat of the errors which may arise from a defect of parallelism in the two surfaces of the index-glass, and to point out the means of obviating them in the celestial observations. It is well known, that if a pencil of parallel rays falls upon a glass whose two

surfaces are inclined to one another, and some of the rays are reflected at the fore-surface, and others passing into the glass and suffering a reflection at the back-surface and two refractions at the fore-surface emerge again from the glass, these latter rays will not be parallel to those reflected at the fore-surface, as they would have been if the surfaces of the glass had been parallel, but will be inclined to the same. I find that the angle of their mutual inclination, which may be called the deviation of the rays reflected from the back-surface, will be to double the inclination of the surfaces of the glass (which is here supposed to be but small), as the tangent of the angle of incidence out of air into glass, is to the tangent of the angle of refraction. Hence, in rays falling near the perpendicular, the deviation will be about three times the inclination of the surfaces; and if the angles of incidence be 50°, 60°, 70°, 80° or 85°, the deviations of the reflected rays will be about 4, 5, 7, 13, or 26 times the inclination of the surfaces, respectively. Had the deviation been the same at all incidences of the rays on the index-glass, no error would have been produced in the observation; because the course of the ray would have been equally affected in the adjustment of the instrument, as in the observation. But, from what has been just laid down, this is far from being the case, the deviation increasing according to the obliquity with which the rays fall upon the index-glass; so that in very oblique incidences of the rays, such as happen in measuring a large angle by the fore-observation or a small angle by the back-observation, the least defect in the parallelism of the planes of the two surfaces of the index glass may produce a sensible error in the observation.

What is here said only takes place in the fullest extent, if the

thickest or thinnest edge of the index-glass, or, to express the same thing in other words, the common section of the planes of the surfaces of the index-glass stands perpendicular to the plane of the quadrant; but, if the common section of the planes is inclined to the plane of the quadrant, the error arising from the defect of the parallelism of the surfaces will be lessened in the proportion of the sine of the inclination to the radius; so that at last, when the common section becomes parallel to the plane of the quadrant, the error entirely vanishes. For this reason: Mr. Hadley very properly directed the thickest and thinnest edges of the index-glass to be placed parallel to the plane of the quadrant. But as it may well be questioned whether this care is always taken by the instrument-maker, and it cannot be supposed that the glasses can be ground perfect parallel planes, it would certainly be an advantage acquired to the instrument, could the error arising from a want of parallelism of the planes be removed in whatever position the common section of the planes should be placed with respect to the plane of the quadrant. This will be effected for celestial observations, if the upper part of the index-glass be left unsilvered on the back, and made rough and blacked, the lower part of the glass being silvered as usual, which must be covered whenever any celestial observations are made. Then, if the telescope be sufficiently raised above the plane of the quadrant, it is evident that the observations will be made by the rays reflected from the fore-surface of the upper part of the index-glass, and consequently, if the quadrant be adjusted by making use of the same part of the index-glass, the observations will be true, whether the two surfaces of the index-glass be parallel planes or not. The sun or moon may

be thus observed by reflection from the unsilvered parts of the index-glass and horizon-glass, so that a paler darkening glass will suffice, and they will appear much distincter than from an index-glass wholly silvered with a deep darkening glass; for although the surfaces of a glass may be parallel, yet there always arises some little confusion from the double reflection. Neither will the moon appear too weak by two unsilvered reflections, even when her crescent is very small, except she should be hazy or clouded; and then the light may be increased by lowering the telescope so as to take in part of the silvered reflection of the index-glass, which in this case must be uncovered: the same is also to be understood with respect to the sun, should his light be too much weakened by haziness or thin clouds. The horizon-glasses should be adjusted, or the error of adjustment found by the sun or moon; the first will be in general the best object for the purpose; and, as the sun or moon seen directly through the unsilvered part of the horizon-glass will be much brighter than the image of the same seen by two unsilvered reflections, it must be weakened by a darkening glass placed beyond the horizon-glass, the reflected image being farther weakened, if necessary, by a paler darkening glass placed in the usual manner between the index-glass and the horizon-glass.

If a quadrant was designed principally for taking the distance of the moon from the sun and fixed stars, and was not wanted for observing terrestrial angles, it would be the best way to have none of the glasses silvered, but to leave the horizon-glasses intirely transparent, and to put a red glass for an index-glass of the same matter with the darkening glasses, which would reflect light from the fore-surface only.

The sun's altitude might also be observed with this instrument, either by the fore or back-observation; and the altitude of the moon might be taken with it in the night. But the altitudes of stars could not be observed with it, nor the moon's altitude in the day time, which would however be no great inconvenience, as these observations might be well enough supplied by common quadrants.

The following rules for the size of the glasses and the silvering them, and the height of the telescope may be of use. The index glass and two horizon-glasses should be all of equal height, and even with one another in height both at top and bottom. The telescope should be moveable parallel to itself nearer to or farther from the plane of the quadrant, and the range of its motion should be such that its axis when at the lowest station should point about $\frac{1}{10}$th of an inch lower than the top of the silvering of the horizon-glasses, and when at the highest station should point to the height of the middle of the unsilvered part of the index-glass. The height of the glasses, and the quantity of parts silvered and parts unsilvered, should vary according to the aperture of the object-glass, as in the following table; where the first column of figures shews the dimensions in parts of an inch answering to an aperture of the object-glass of $\frac{3}{10}$ths of an inch in diameter; the second column what answer to an aperture of the object-glass of $\frac{4}{10}$ths of an inch in diameter; and the third, what are suitable to an aperture of the object-glass of $\frac{5}{10}$ths of an inch in diameter.

	Parts of an Inch.		
Diameter of aperture of object-glasss	,30	0,40	0,50
Height of glasses	,90	1,13	1,37
Height of silvered part of index glass	,50	0,63	0,77
Height of unsilvered part of ditto	,40	0,50	0,60
Height of silvered part of horizon-glasses	,25	0,33	0,42
Height of unsilvered part of ditto	,65	0,80	0,95

If the telescope has a common object-glass, the first aperture of $\frac{3}{10}$ths of an inch will be most convenient; but if it has an achromatic object-glass, one of the other apertures of $\frac{4}{10}$ths or $\frac{5}{10}$ths of an inch, will be most proper. The field of view of the telescope should be 5 or 6 degrees, and the objects should be rendered as distinct as possible throughout the whole field, by applying two eye-glasses to the telescope. The breadth of the glasses should be determined as usual, according to the obliquity with which the rays fall on them and the aperture of the object-glass.

I shall conclude this paper with some easy rules for finding the apparent angular distance between any two near land objects by the Hadley's quadrant.

To find the angular distance between two near objects by the fore-observation. Adjust the fore-horizon-glass by the object intended to be taken as the direct-object; and the angle measured by the fore-observation on the arch of the quadrant between this object and any other object seen by reflection will be the true angle between them as seen from the centre of the index-glass. But, if the quadrant be already well adjusted by a distant object, and you do not chuse to alter it by adjusting it by a near one, move the index, and bring the image

of the near direct object to coincide with the same seen directly, and the number of minutes by which (0) of the index stands to the right hand of (0) of the quadrant upon the arch of the excess is the correction, which added to the angle measured by the arch of the quadrant between this direct object and any other object seen by reflection will give the true angular distance between them reduced to the centre of the index.

To find the angular distance between two near objects by the back-observation.

It is supposed that the horizon-glass is truly adjusted; if it is not, let it be so. Observe the distance of the objects by the back-observation, and take the supplement of the degrees and minutes standing upon the arch to 180 degrees, which call the instrumental angular distance of the objects; this is to be corrected as follows. Keep the centre of the quadrant or index-glass in the same place as it had in the foregoing observation, and observe the distance between the near object, which has been just taken as the direct object, and some distant object, twice; by making both objects to be the direct and reflected ones alternately, holding the divided arch upwards in one case and downwards in the other, still preserving the place of the centre of the quadrant. The difference of these two observations will be the correction, which added to the instrumental angular distance, found as above in the first observation between the first object and any other object seen by reflection, will give the true angular distance between them reduced to the centre of the index glass.

But if you should happen to be in a place where you cannot command a convenient distant object, the following method may be used. The back-horizon-glass being adjusted, find the instrumental angular distance between the objects; this is to be corrected by means of the following operations. Set up a mark at any convenient distance opposite or nearly so to the object which has been taken as the direct object; and looking at the direct object move the index of the quadrant, and bring the image of the mark to coincide with the direct object, and read off the degrees and minutes standing on the arch of the quadrant, which substract from 180 degrees, if (0) of the index falls upon the quadrantal arch; but add to 180 degrees, if it falls upon the arch of excess; and you will have the instrumental angular distance of the object and mark. Invert the plane of the quadrant, taking care at the same time not to change the place of its centre, and looking at the same direct object as before, move the index of the quadrant, and bring the image of the mark to coincide again with the direct object, and read off the degrees and minutes standing on the arch, and thence also find the instrumental angular distance of the object and mark. Take the sum of this and the former instrumental angular distance; half of its difference from 360° will be the correction, which added to the instrumental angular distance first found between the same direct object and the other object seen by reflection will give the true angular distance between them reduced to the centre of the index-glass.

It is to be observed, that if the mark be set up at the same distance from the quadrant as the direct object is, there will be no occasion to invert the plane of the quadrant, but the observer need only make

the image of the mark coincide with the direct object, then turn himself half round, and now taking the mark for the direct object cause the image of the former direct object to coincide with the mark, the divided arch of the quadrant being kept upwards, and the place of the centre of the quadrant remaining also the same in both cases: half the difference of the sum of the two instrumental angles from 360° will be the correction of the adjustment as before.

Should only one of the objects be near, and the other remote (that is to say, half a mile distant or more) let the distant object be taken for the direct one, and the near object for the reflected one; and the true distance of the objects as seen from the centre of the index-glass will be obtained without requiring any correction, whether it be the back or fore-observation that is made use of; only observing, as usual, to take the supplement of what is shown upon the arch to 180° in the back-observation.

An Account of an Apparatus applied to the Equatorial Instrument for correcting the Errors arising from the Refraction in Altitude. By Mr. Peter Dollond, *Optician; communicated to the Royal Society by the Astronomer Royal.*

Read March 4, 1779.

THE refraction of the atmosphere occasions the stars or planets to appear higher above the horizon than they really are; therefore, a correction for this refraction should be made in a vertical direction to the horizon.

The equatorial instrument is so constructed, that the correction cannot be made by the arches or circles which compose it, when the star, &c. is in any other vertical arch except that of the meridian; because the declination arch is never in a vertical position but when the telescope is in the plane of the meridian.

To correct this error, a method of moving the eye-tube which contains the wires of the telescope in a vertical direction to the horizon has been practised; but as the eye-tube is obliged to be turned round in order to move it in that direction, in the different oblique positions of the instrument, the wires are thereby put out of their proper situation in every other position of the instrument, except when it is in the plane of the meridian; for the equatorial wire should always be parallel to the equator, that the star in passing over the field of the telescope may move along with it, otherwise one cannot judge whether the telescope be set to the proper declination, except at the instant the star is brought to the intersection of the wires, which is only a momentary observation.

The method I have now put in practice for correcting the refraction of the atmosphere is, by applying two lenses before the object-glass of the telescope; one of them convex, and the other concave; both ground on spheres of the same radius, which in those I have made is thirty feet. The convex lens is round, of the same diameter as the object-glass of the telescope, and fixed into a brass frame or apparatus, which fits on to the end of the telescope. The concave lens is of the same width, but nearly two inches longer than it is wide, and is fixed in an oblong frame, which is made to slide on the frame that the other lens is fixed into, and close to it. These two lenses being wrought on spheres of the same radius, the refraction of the one will be exactly destroyed by that of the other, and the focal length of the object-glass will not be altered by their being applied before it: and if the centres of these two lenses coincide with each

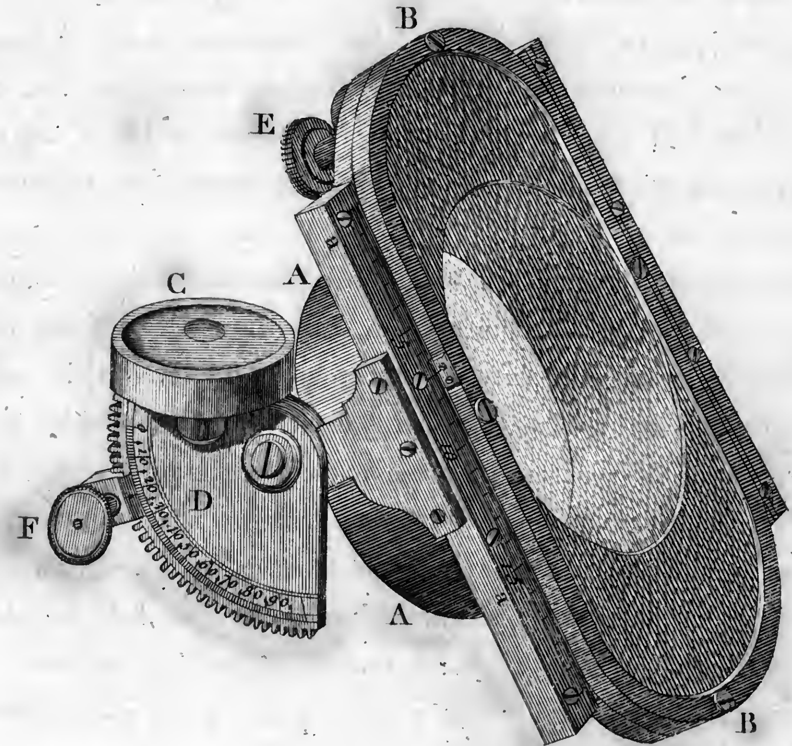
other, and also with that of the object-glass, the image of any object formed in the telescope will not be moved or suffer any change in its position. But if one of the lenses be moved on the other, in the direction of a vertical arch, so as to separate its centre from that of the other lens, it will occasion a refraction, and the image will change its altitude in the telescope. The quantity of the refraction will be always in proportion to the motion of the lens, so that by a scale of equal parts applied to the brass frame, the lens may be set to occasion a refraction equal to the refraction of the atmosphere in any altitude. If the concave lens be moved downwards, that is, towards the horizon, its refraction will then be in a contrary direction to that of the atmosphere, and the star will appear in the telescope as if no refraction had taken place.

There is a small circular spirit level fixed on one side of the apparatus, which serves to set it in such a position, that the centres of the two lenses may be in the plane of a vertical arch. This level is also used for adjusting a small quadrant, which is fixed to it, and divided into degrees, to shew the elevation of the telescope when directed to the star; then the quantity of refraction answering to that altitude may be found by the common tables, and the concave lens set accordingly, by means of the scale at the side, which is divided into half minutes, and, if required, by using a nonius, may be divided into seconds.

It must be observed, that when a star or planet is but a few degrees above the horizon, the refraction of the atmosphere occasions it to be considerably coloured. The refraction of the lens acting in a contrary direction would exactly correct that colour, if the dissipation of the

rays of light were the same in glass as in air; but as it is greater in glass than in air, the colours occasioned by the refraction of the atmosphere will be rather more than corrected by those occasioned by the refraction of the lens.

The following is a drawing of the refraction apparatus, which may serve to give a more clear idea of it.



EXPLANATION OF THE PLATE.

AA. The circular brass tube, which fits on to the end of the telescope.

BB. The oblong concave lens in its frame, which slides over the fixed convex lens.

C. The circular spirit level, which shews when the oblong lens is in a vertical arch.

D. The quadrant to which the spirit level is fixed, for shewing the angular elevation of the telescope.

E. The milled head fixed to a pinion, by which the whole apparatus is turned round on the end of the telescope, in order to set the oblong lens in a vertical arch.

F. Another pinion for setting the quadrant to the angular elevation of the telescope. By means of these two pinions the air bubble must be brought to the middle of the level.

aa. Is the scale, with divisions answering to minutes and half minutes of the refraction occasioned by the concave lens.

FINIS

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